APR -3 1961

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Vol. 133, No. 3456

SCIENCE

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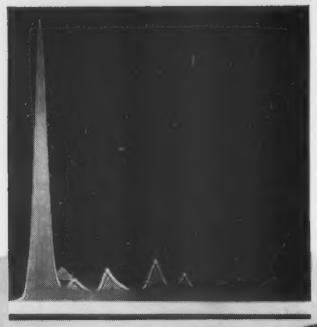
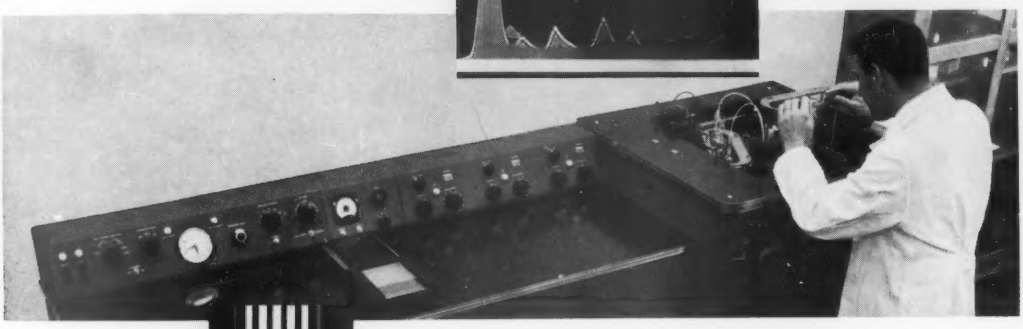
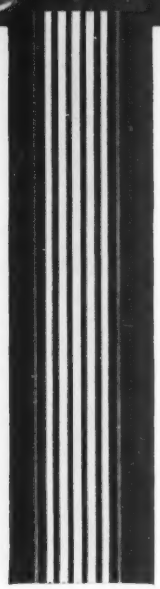
Gordon Research Conferences



Electrophoresis of human plasma diluted 1:6; ascending boundaries. Inclined knife-edge schlieren.





Portion of typical reference fringe pattern obtained from standard production model, magnified to show straightness and definition of entire pattern.

ELECTROPHORESIS AND DIFFUSION

in one precision instrument

As protein research progresses, biochemists rely more and more upon instruments of high precision for diffusion and electrophoresis studies. Especially critical are the optical measurements needed to obtain accurate diffusion coefficients, absolute electrophoretic mobilities, and information on purity.

An exceptional optical system is one of the outstanding features which have made the Spinco Model H invaluable for exacting work in both electrophoresis and diffusion. Light passes through each operating cell twice, giving double sensitivity. Patterns are sharply defined and peak positions can be precisely determined. Reproducible measurements may be made to better than 1/25 of a fringe, which corresponds to approximately .00025 percent protein.

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Sales and service facilities on the Model H are available on the same basis as for Spinco Ultracentrifuges, assuring prompt, efficient service for users here and abroad.

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Hydroxylamine-O-Sulfonic Acid. (H_2NOSO_3H)
Duroyl Propionic Acid. ($C_{10}H_{13}COCH_2CH_2COOH$)
2, 2-Dichlorobicyclo- (4,1,0) Heptane. ($C_7H_{10}Cl_2$)
2-Methylmercaptophenol. ($CH_3SC_6H_4OH$)
4-Methylmercaptophenol. ($CH_3SC_6H_4OH$)
Sulfamide. ($NH_2SO_2NH_2$)
Sulfuryl Fluoride. (SO_2F_2)
Sulfur Trioxide-Pyridine Complex. ($SO_3 \cdot$ Pyridine)
Methylene Sulfate. ($CH_2SO_4)_2$

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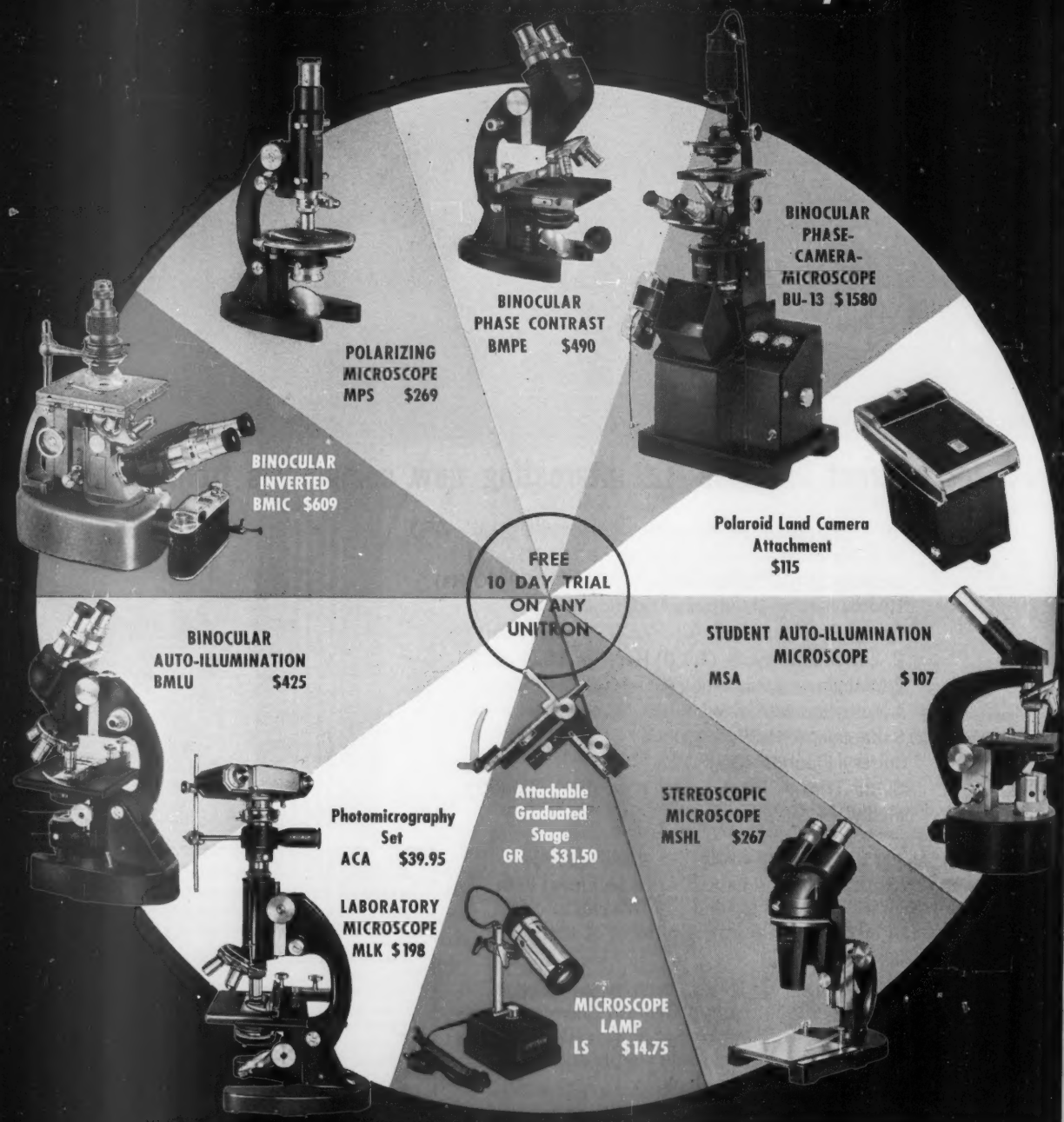

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24 March 1961, Volume 133, Number 3456

SCIENCE

Cover

Oblique aerial view of the Wabar meteor crater, about 300 feet in diameter and 40 feet deep, near Al Hadida, in east-central Saudi Arabia. See page 882. [Arabian-American Oil Company]

measure the volume of a pulse beat . . . the weight of a breath . . . the vacuum of outer space
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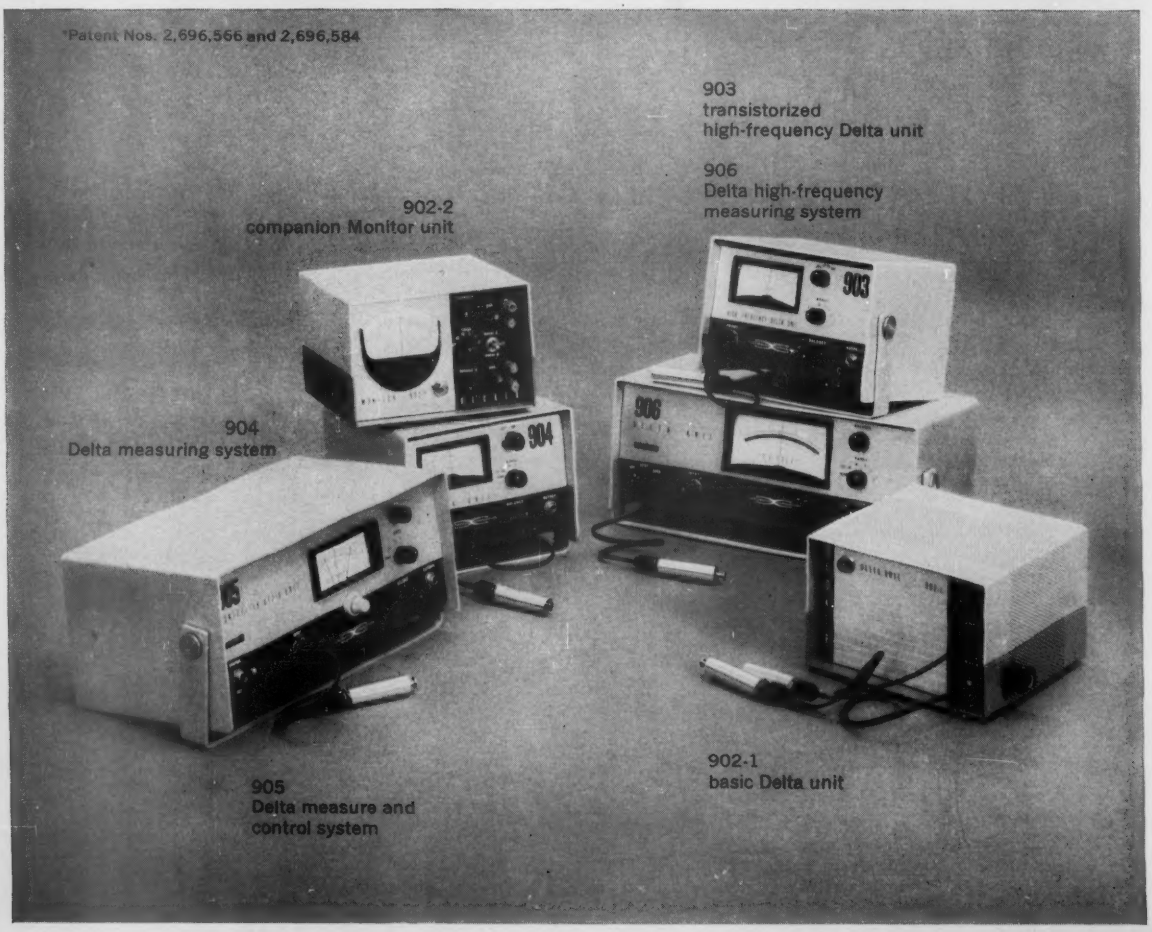

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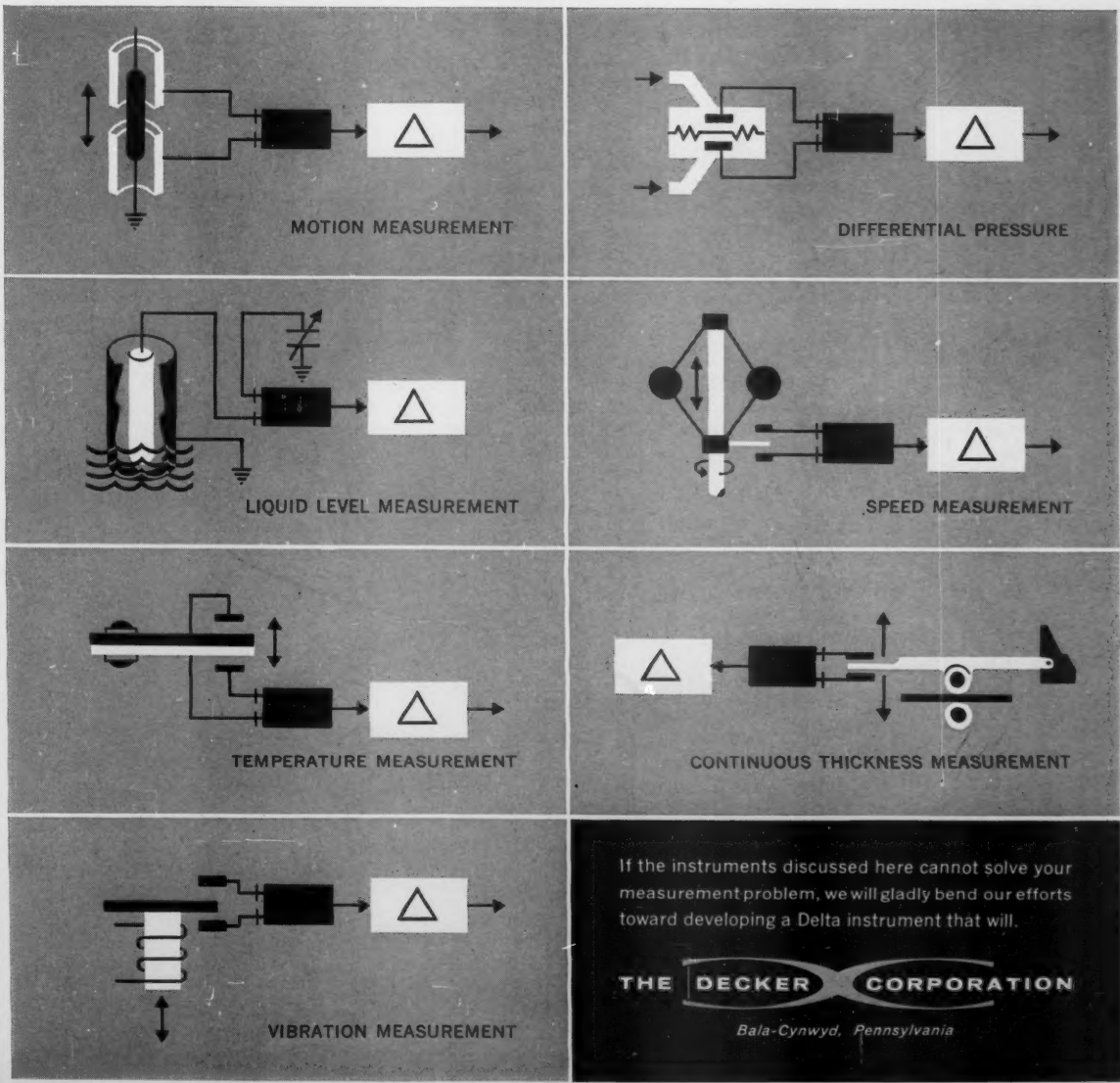

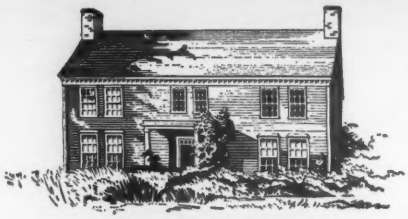

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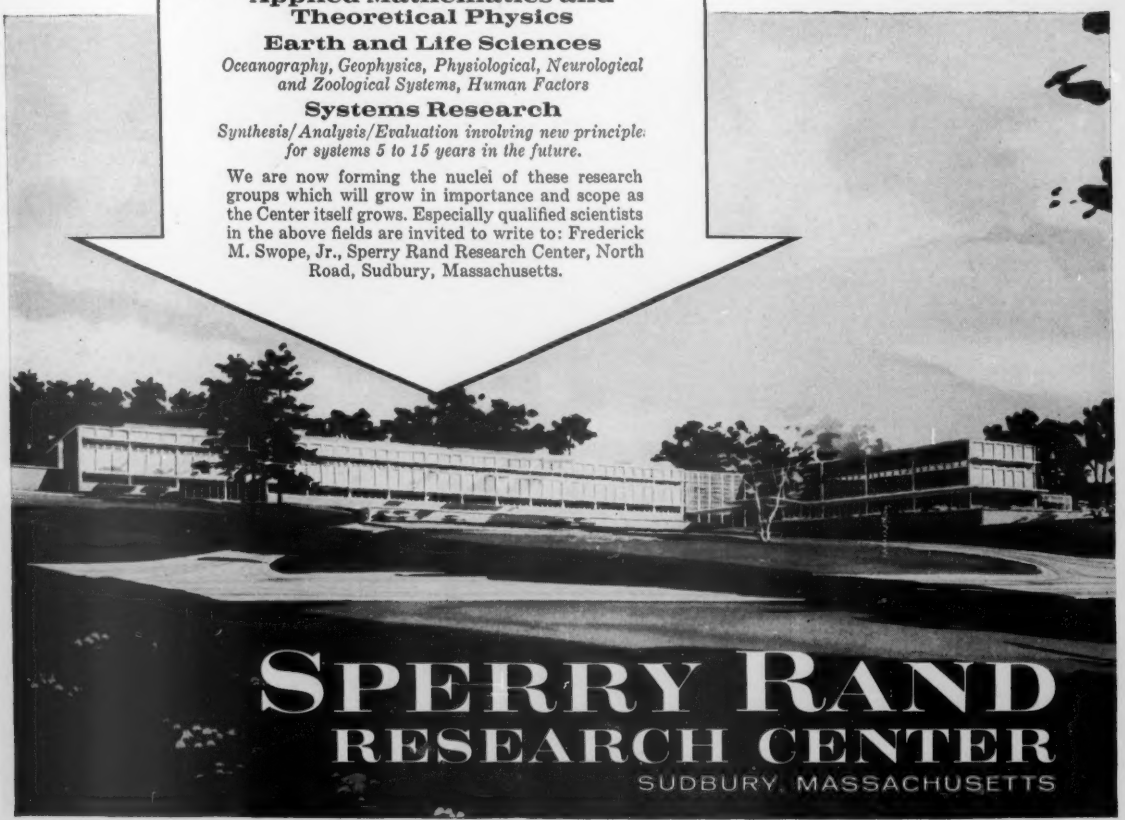

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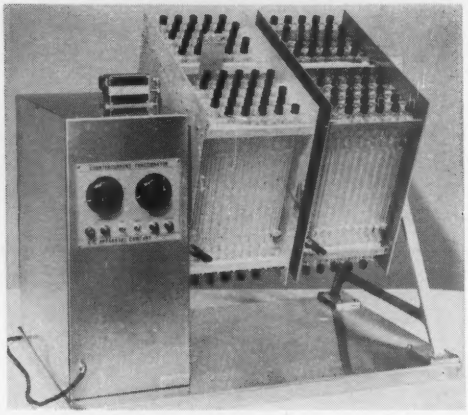

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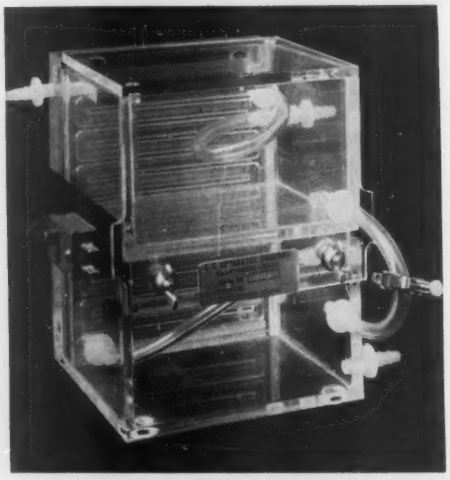
VERTICAL GEL ELECTROPHORESIS



EC470 VERTICAL GEL ELECTROPHORESIS APPARATUS.

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ANALYTICAL and *PREPARATIVE METHODS* of *SEPARATION. GEL ELECTROPHORESIS*

Electrophoresis on gel supporting media affords much improved resolution in comparison with paper-strip or free-solution electrophoresis. As many as 24 distinctly-separated components can be obtained from serum samples. Altho equally suitable for micro quantities, gel electrophoresis also accepts much larger quantities making it a practical method for preparative separations on a semi-micro scale; up to 200 mg. of protein mixture can be separated on one gel slab. If direct-contact cooling of the gel slab is employed, complete serum protein patterns can be obtained in less than two hours, showing better resolution than a 20-hour pattern on paper electrophoresis.

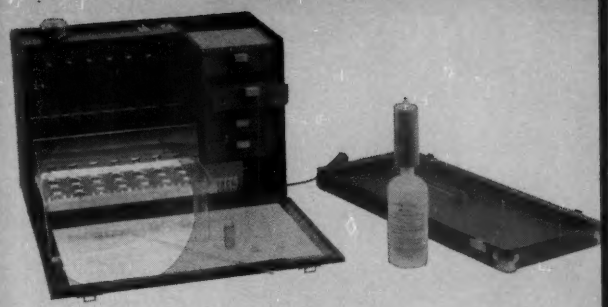
The gel material used as supporting medium may be hydrolyzed starch, agar, or the synthetic polyacrylamide Cyanogum (Reg. American Cyanamid Co.). Each of these media gives a different type of resolution, and in many problems it will be advantageous to employ all three concurrently or successively; the procedure then becomes analogous to two-dimensional chromatography. Migration thru a gel slab in the horizontal position introduces undesirable gravitational-convection effects in the migrating zone; therefore, a vertical gel slab is preferable, providing that a proper supporting apparatus is available.

COUNTERCURRENT DISTRIBUTION

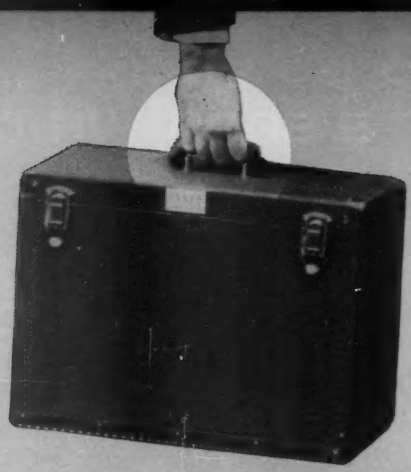
Counter-current extraction is a method of analyzing or separating the components of a mixture by dissolving it in one phase of a two-phase solvent system, and extracting it with the other phase. This effects a relative separation of the components according to the distribution co-efficients of each. Repeating the extraction many times in a systemic manner multiplies the effects of even small differences in distribution coefficients, so that closely similar substances can be separated. The separated volumes may be analyzed by any applicable method, preferably one which gives the total quantity of solute in each volume. Thus the total volume of solvent may be titrated when dealing with a mixture of acids. The most generally applicable, when non-volatile solutes are concerned, is the determination of the weight of solute in each fraction. It should be noted, however, that macro quantities of material are not required. Extremely low concentrations may be used, which may be far below those required for weight determinations. Necessary, of course, are sensitive analytical methods, such as color reactions or ultraviolet absorption. With such methods, counter-current extraction can analyze quantities as small as those in any other analysis. Results using low concentrations may be even more accurate than those with high concentrations owing to a closer approach to the laws of ideal solutions from which the following equations are derived. Counter-current extraction follows very exactly the Distribution Law

$$Ki = \frac{(i)1}{(i)2} \qquad (1)$$

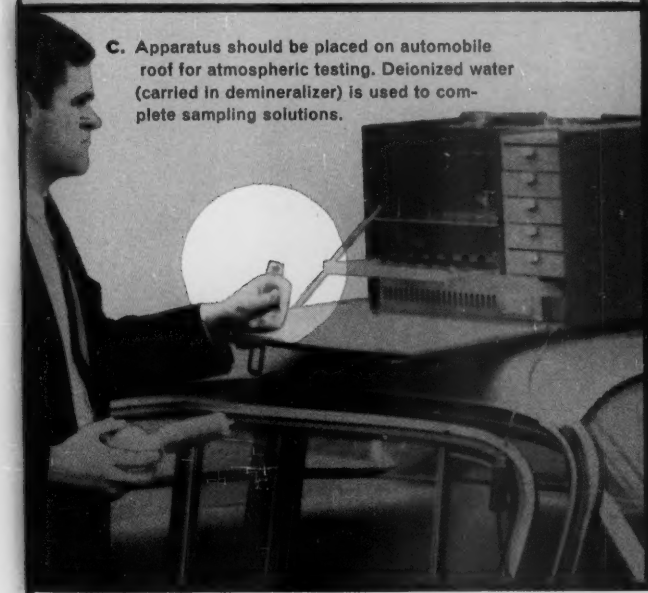
where K_i is a constant characteristic of the compound i; $(i)_1$, and $(i)_2$ are the concentrations of compound i at equilibrium in phase 1 and phase 2 of the solvent system. This fact allows a very mathematical analysis of the quantitative results obtained in counter-current extraction, and permits the direct comparison of experimental and theoretical results.



A. Air Pollution Apparatus ready for survey. Illuminated filter slide permits direct, quantitative determination of gaseous pollutants.



B. Lightweight, compact design makes it easy to transport to test site. Even untrained personnel can make initial studies with the Cenco Test Apparatus.



C. Apparatus should be placed on automobile roof for atmospheric testing. Deionized water (carried in demineralizer) is used to complete sampling solutions.

D. Colorimeter is used to estimate gaseous pollutants. Cenco Field Test Apparatus can test over 18 major pollutants.

NEW AIR POLLUTION FIELD TEST APPARATUS

You can make on-the-spot surveys of 18 major pollutants in just minutes. This new Air Pollution Test Apparatus is ideal for detection where fast accurate information is needed. It can also be used to make initial studies of previously unsurveyed areas prior to the setting up of a monitoring program. Trained personnel are not required for operation. The Field Test Apparatus is portable, lightweight and easy to set up and use. It was developed by Kem-Tech Laboratories, Inc. of Baton Rouge, La., for the U. S. Department of Health, Education and Welfare. For details, write for Booklet 313.

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Additional operational features include an average add execution time of three microseconds, and a memory cycle time of six microseconds. Both character- and word-oriented, the B 5000 operates in binary *and* alphanumeric modes; a single set of arithmetic commands operates interchangeably on both fixed-point and floating-point numbers.

More important than these features is the fact that they combine with compiler-oriented logic and language to provide a new concept in computing—an integrated hardware-software system which sets:

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Incorporating logic and language designed to take advantage of modern compiler techniques, the B 5000 permits straightforward, efficient translation of common-language source programs. And it brings a new high in compilation speeds—20 to 50 times faster than those possible on conventional computer systems.

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Availability of multiple, functionally independent modules provides the B 5000 with excellent system flexibility and expansibility. The system may include one or two independent processors; up to eight core memory modules with a total capacity of 32,768 48-bit words; and one or two fast-access bulk storage drums, each with a capacity of 32,768 words. Up to four independent input/output channels control a maximum of 26 input/output units, including up to 16 standard-format magnetic tape units. Additional input/output units include card punch and reader, two types of printer, plotter and keyboard.

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The new B 5000 permits simultaneous on-line/off-line operation. It features completely flexible communication among all of its units. A central processor communicates with all memory units. Any input/output channel communicates with any peripheral equipment and any memory module.

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For details in depth on the B 5000, call our nearby office. Or write for a copy of "The B 5000 Concept" to Data Processing Division, Burroughs Corporation, Detroit 32, Michigan.

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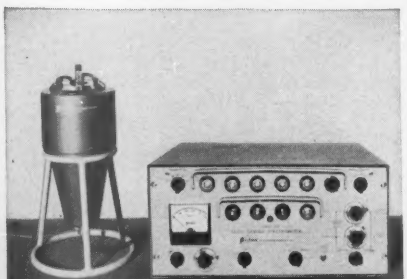

For complete information write for Bulletin 400.

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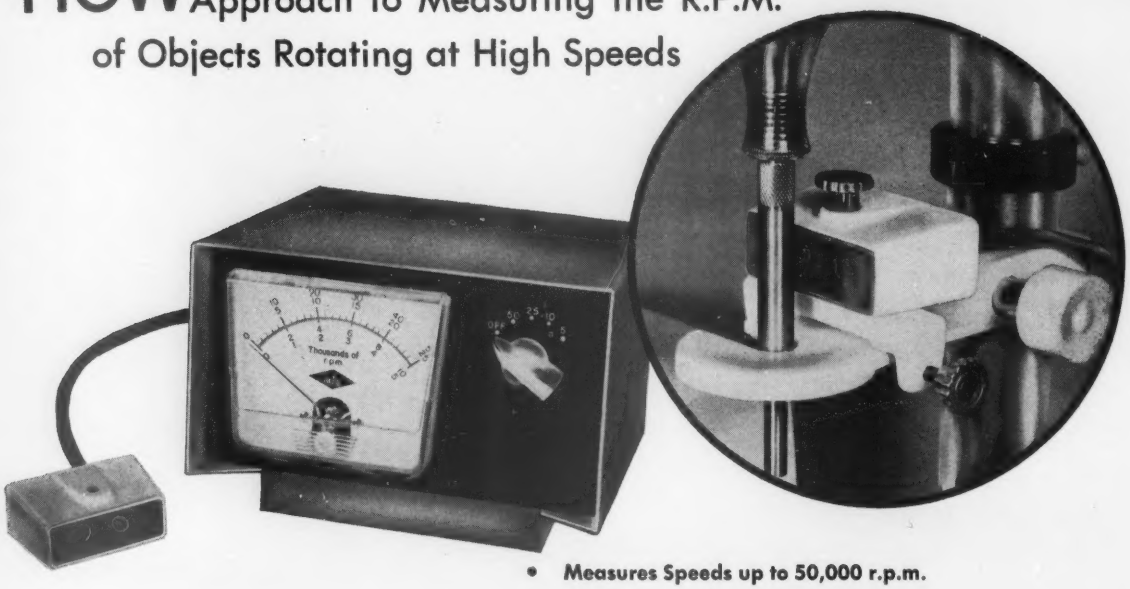

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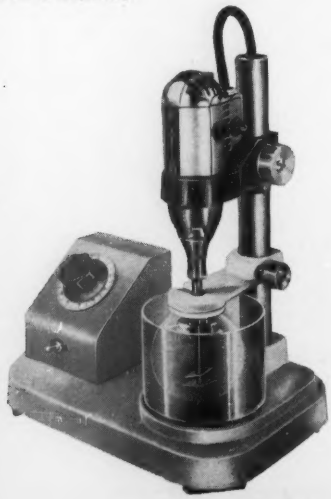

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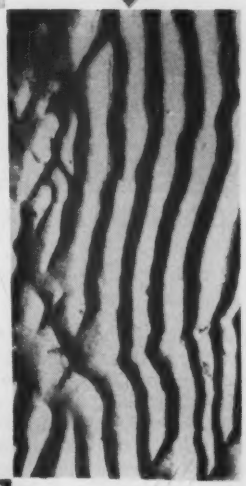

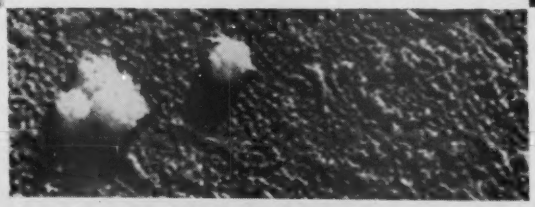

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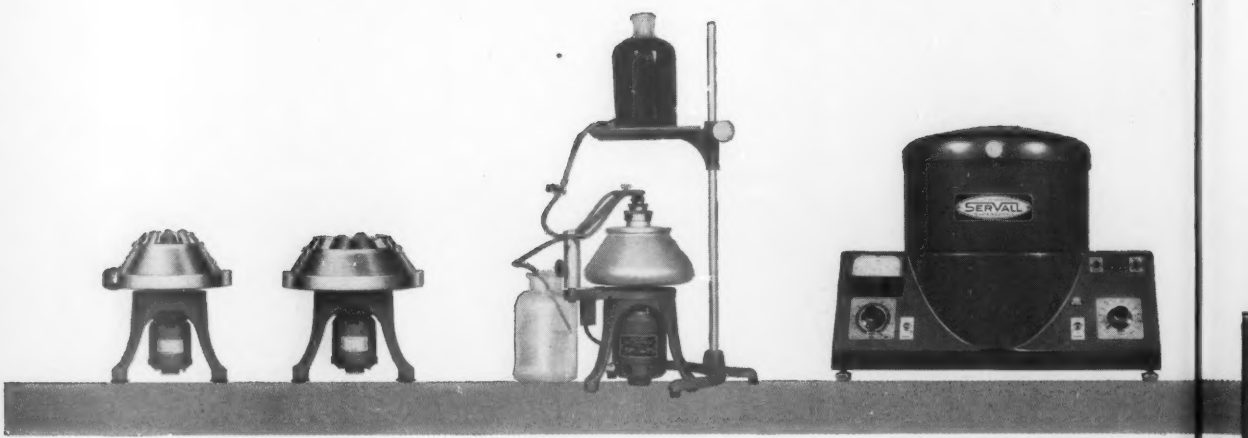

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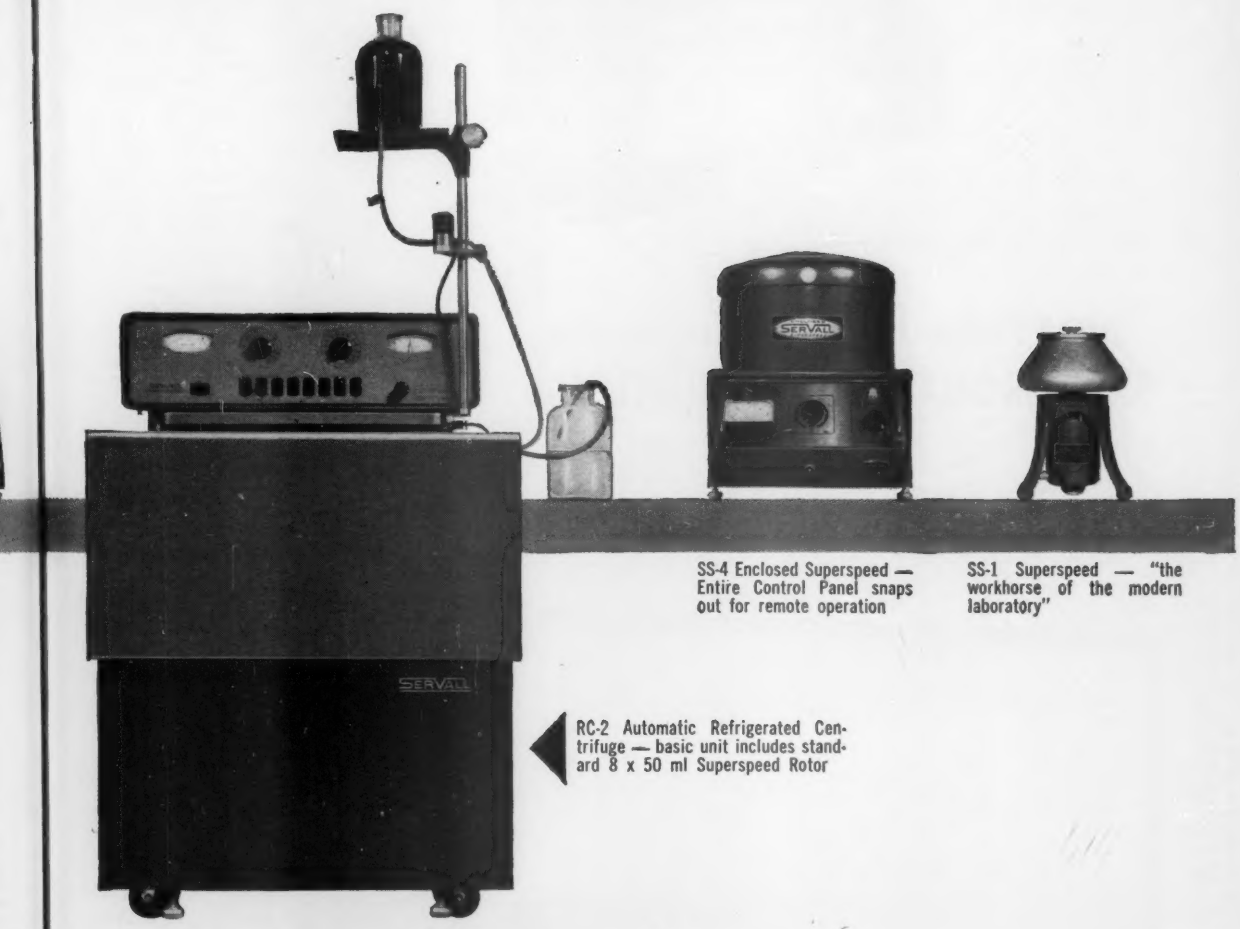

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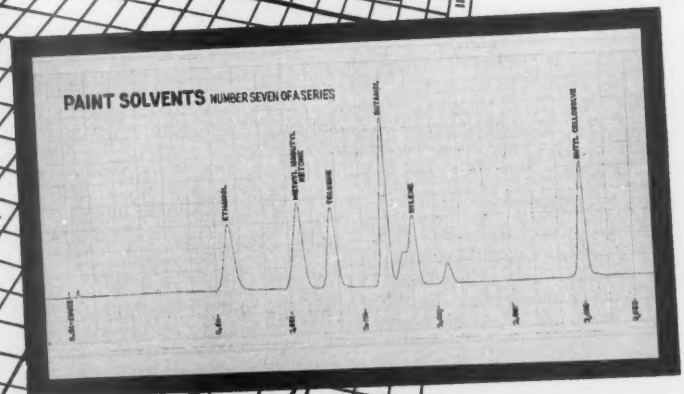

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LITERATURE CITED

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(6.) Technical Bulletin, SC:60-137, Shell Chemical Company, "The Application of Gas Liquid Chromatography in Surface Coating Research," October 1960.

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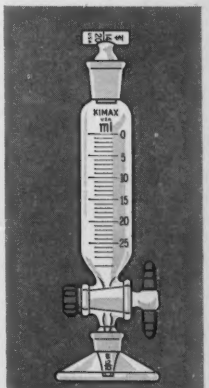

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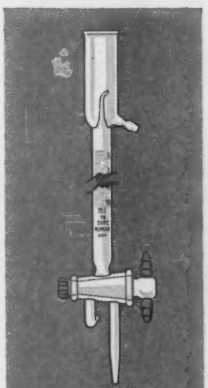

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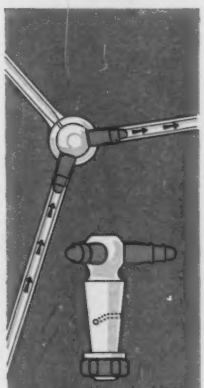

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EXAMPLE

Electrolytic production of negative ions.

Figure 1
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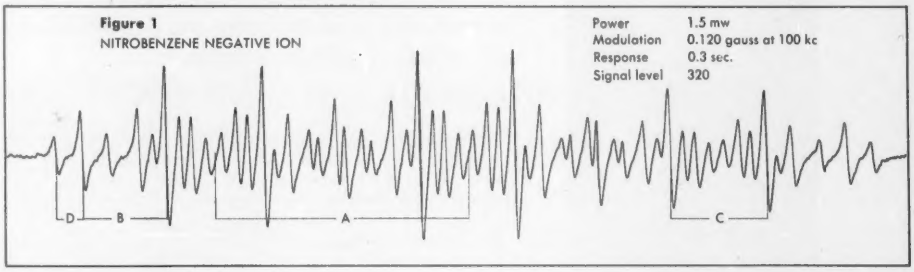
Response	0.3 sec.
Signal level	320



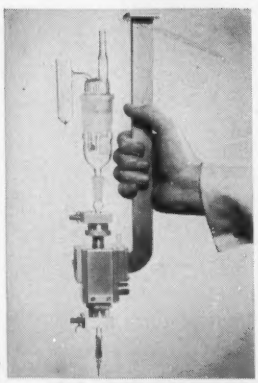


Figure 2 Electrochemical cell as used with the spectrometer

Recently Maki and Geske[1] reported a radically new and important application of EPR. They showed that it was now possible to observe directly the one electron transfer process in the electrolytic reduction of nitrobenzene to the negative ion. They prepared the negative ion by constant potential electrolysis of nitrobenzene in a solution of acetonitrile with tetra-n-propylammonium perchlorate as supporting electrolyte. Such methods of production of negative ions are preferable to the metal reduced systems in that the EPR spectrum can be interpreted completely without complication of interaction by the metal.

Figure 1 shows the spectrum of the nitrobenzene negative ion when generated from a solution of benzonitrile and tetra-n-propylammonium perchlorate[2]. Forming the ion in benzonitrile seems to improve the resolution of the spectrum obtained. The predicted 54 lines are easily observed.

Splitting (A) represents the nitrogen coupling constant which is 10.3 gauss. Splitting (B), (C) and (D) correspond to the coupling constants of the ortho, para and meta hydrogens. The electrochemical cell used in the generation of the negative ions is illustrated in Figure 2 and is a modification of the V-4548 aqueous sample cell accessory.

[1] *JACS 82, 2671 (1960).*
[2] *Sample donated by Dr. R. Adams, University of Kansas.*

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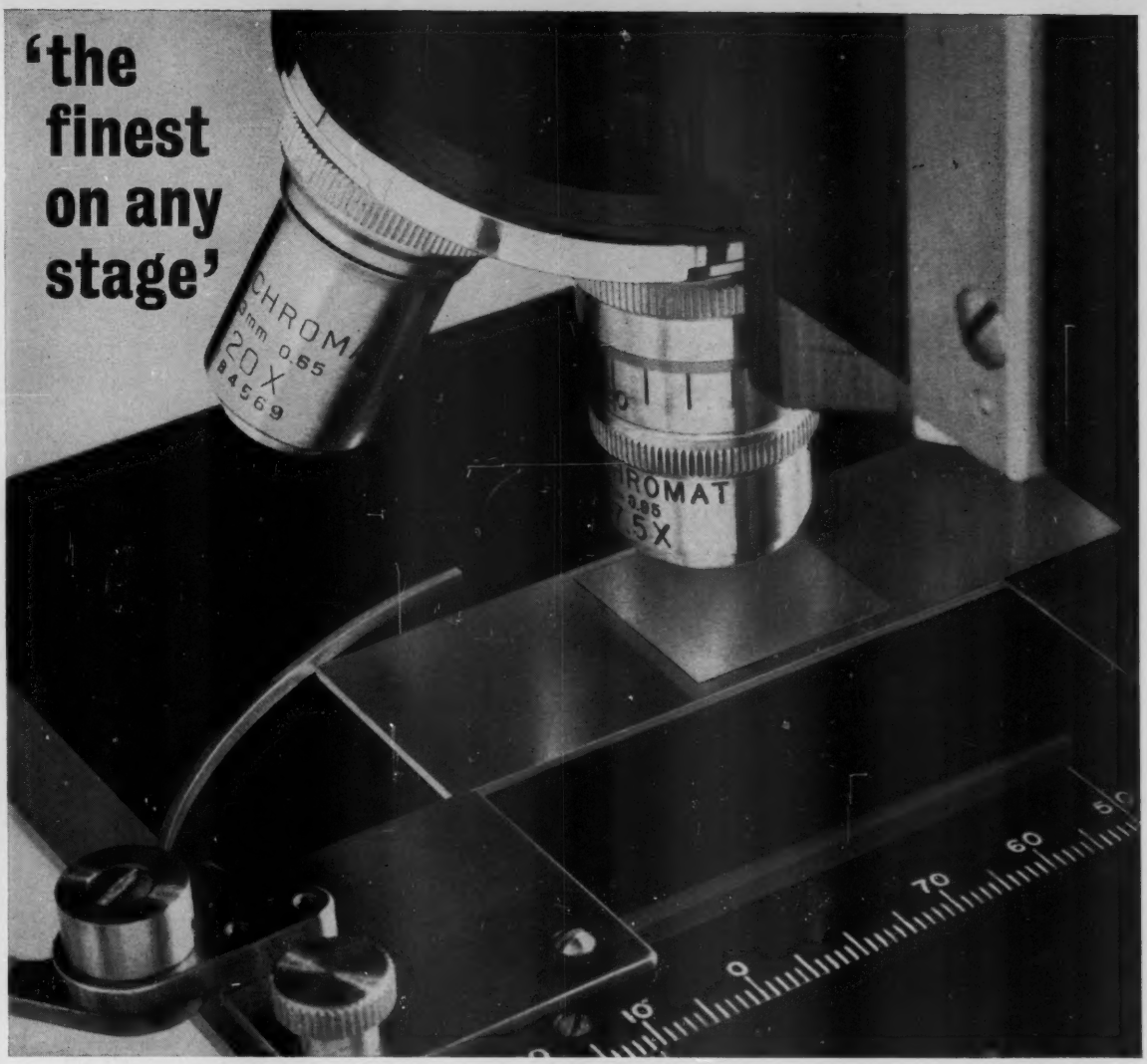

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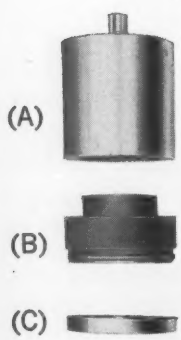

Exploded view of the FD Series Flow Counter showing (A) Counter Shield (B) Plug-in Flow Counter (C) Snap-On Window.

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FD-2**	Geiger	G-1	.25V	1450V ± 150	<30 c.p.m.	300V	<2%
FD-1	Prop.	G-2	α 10 mv	2200V ± 500	α < 6 c.p.h.	1000V	<2%
			β 1 mv	2300V ± 200	β <12 c.p.m.	400V	<2%
FD-1	Prop.	Pure Methane	α 10 mv	3500V ± 500	α < 6 c.p.h.	1000V	<1%
			β 10 mv	4500V ± 500	β <12 c.p.m.	1000V	<1%
FD-1	Prop.	Natural Gas	α 10 mv	4000V ± 500	α < 6 c.p.h.	1000V	<2%
			β 10 mv	4700V ± 300	β <12 c.p.m.	400V	<2%
FD-2	Prop.	G-2	α 10 mv	2200V ± 500	α <12 c.p.h.	1000V	<2%
			β 1 mv	2300V ± 200	β <30 c.p.m.	400V	<2%
FD-2	Prop.	Pure Methane	α 10 mv	3000V ± 500	α <12 c.p.h.	1000V	<1%
			β 10 mv	4500V ± 500	β <30 c.p.m.	1000V	<1%
FD-2	Prop.	Natural Gas	α 10 mv	4000V ± 500	α <12 c.p.h.	1000V	<2%
			β 10 mv	4700V ± 300	β <30 c.p.m.	600V	<2%

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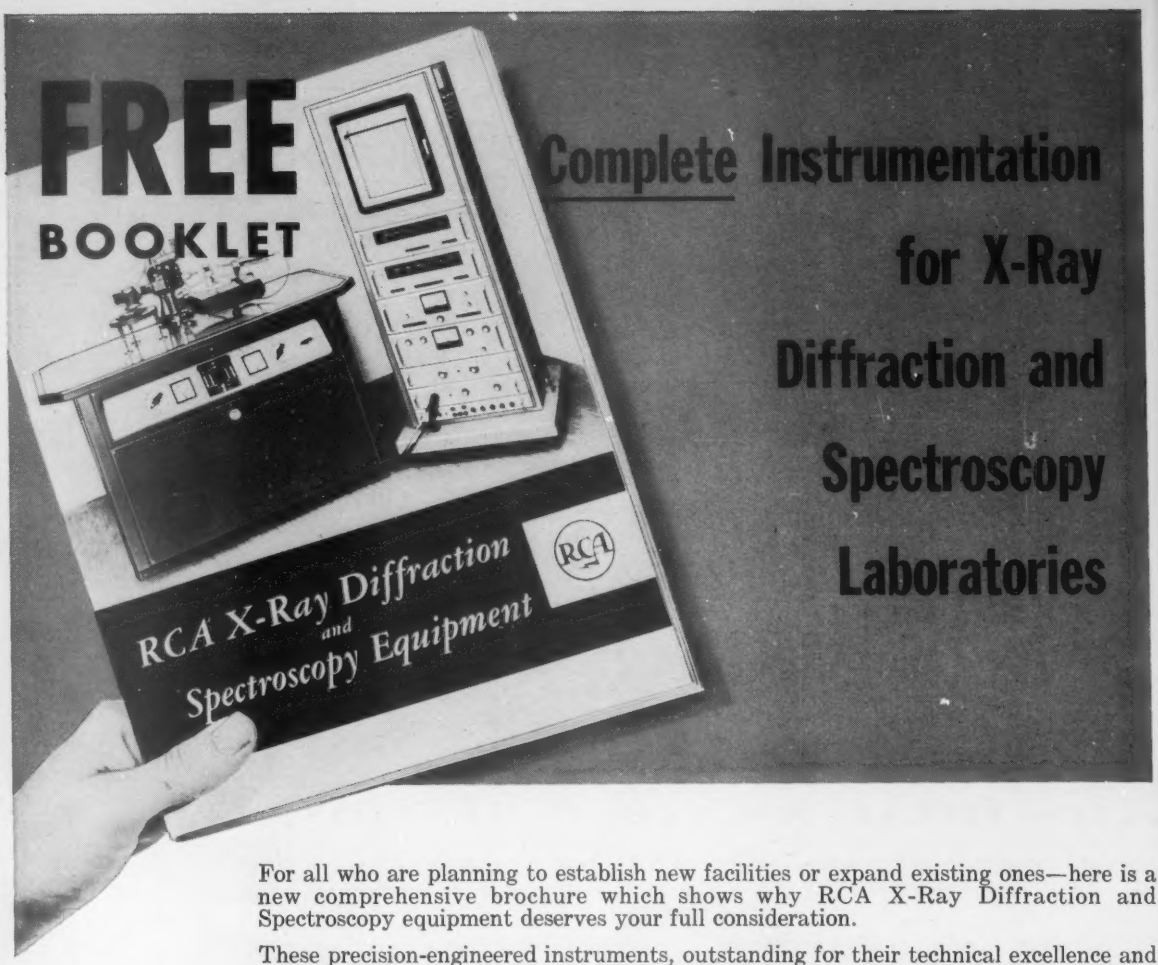

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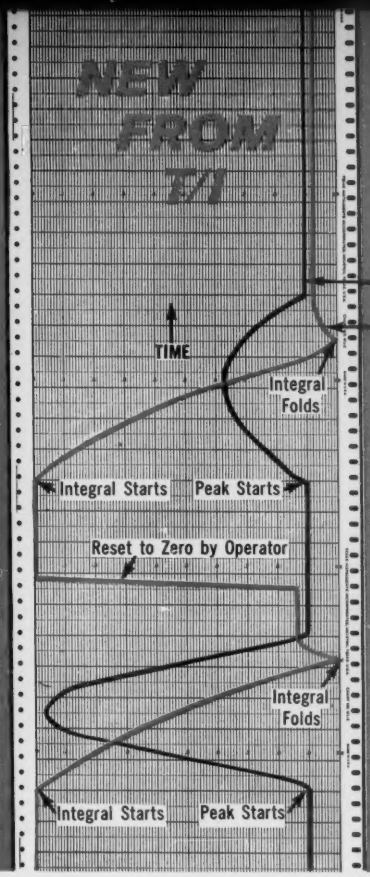

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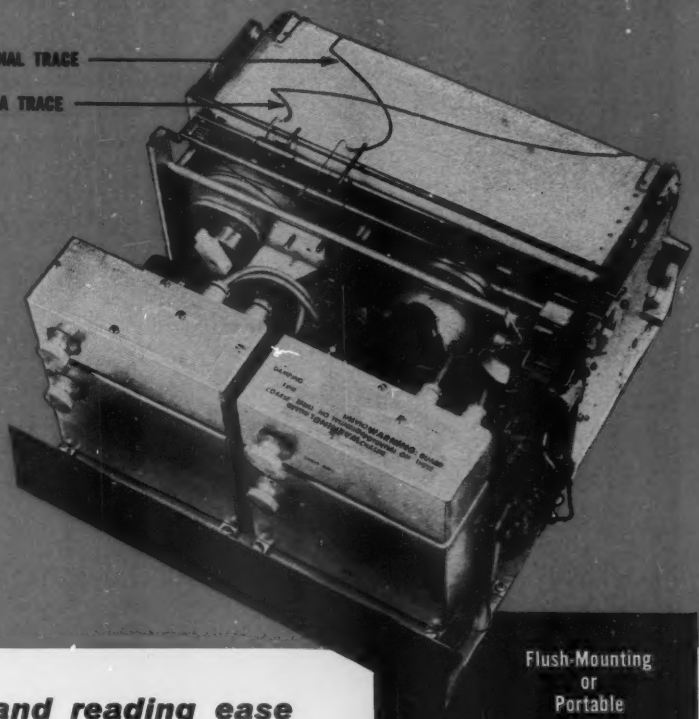

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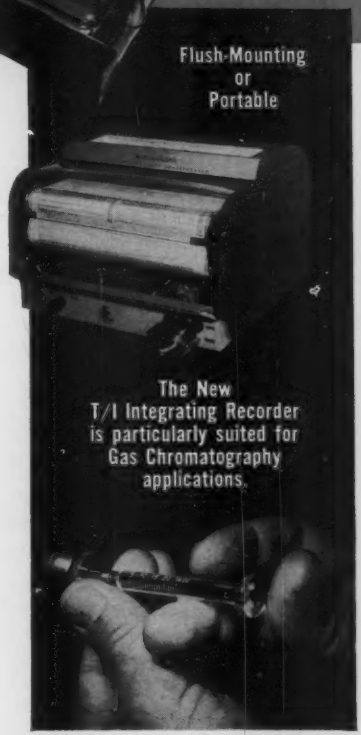

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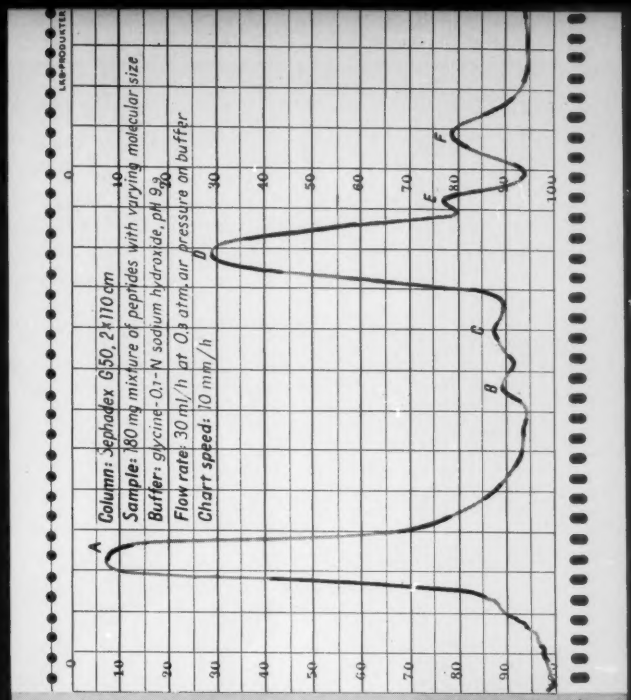

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C	4	—
D	39	2.7×10^4
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Volume of Cell	0.05 ml	0.1 ml	0.1 ml	0.2 ml	0.3 ml

* **HIGH SENSITIVITY AND WIDE MEASURING RANGE**
Example 1. 0.00036 % thyrosine in 0.1-N NaOH, 5 mm cell ≃ 10 mm pen deflection (E = 0.05)
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Example 11. 0.02 % serum albumin in 1-% NaCl, 5 mm cell ≃ 10 mm pen deflection (E = 0.05).

* **LOW UV DOSE ON SENSITIVE MATERIAL**
Absorbed UV energy is approx. 1 milliwatt-second per ml at 10 ml/hour. This corresponds to a deterioration factor in bovine serum albumin of only 0.0001.

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Drift is less than 5 % (transmission) in 24 hours and less than 5 % for room-temperature changes of 15 °C, either from 15 to 30 °C, or with coldroom attachment from 0 to 15 °C.

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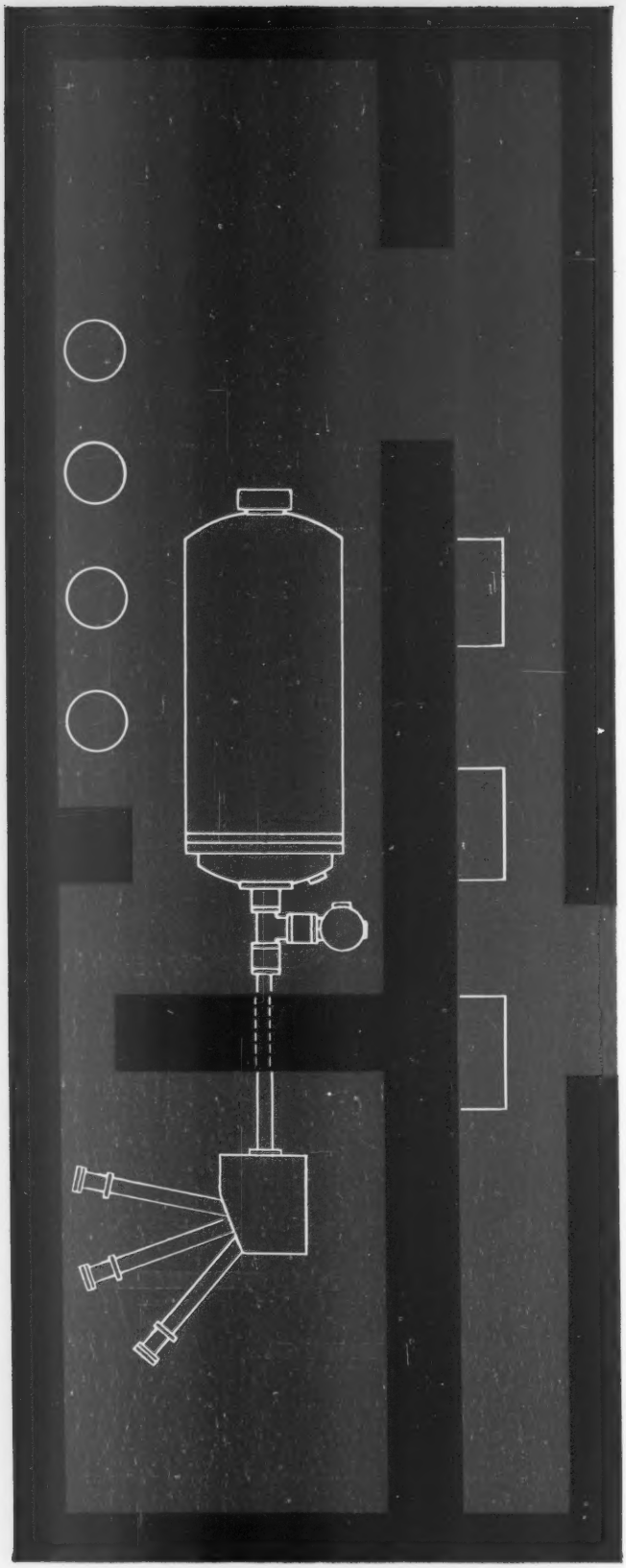

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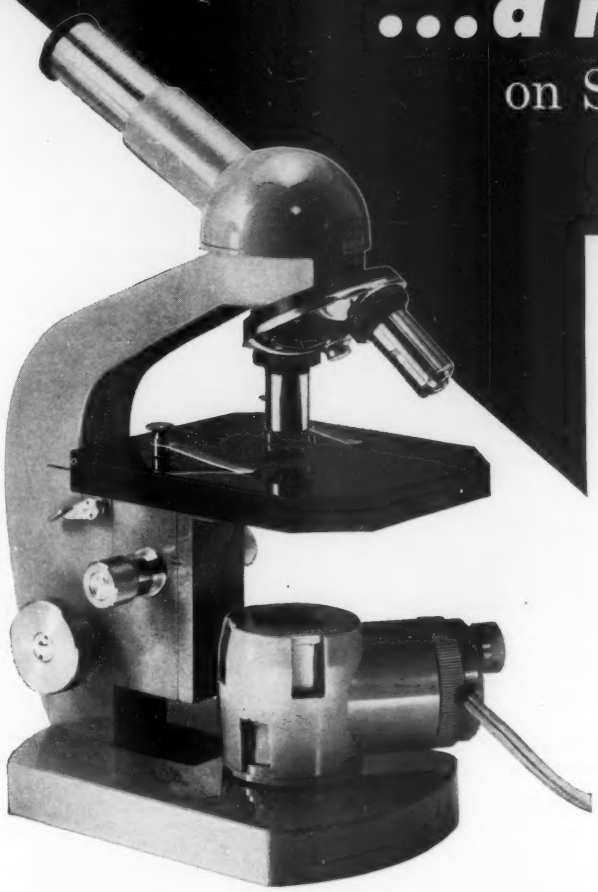

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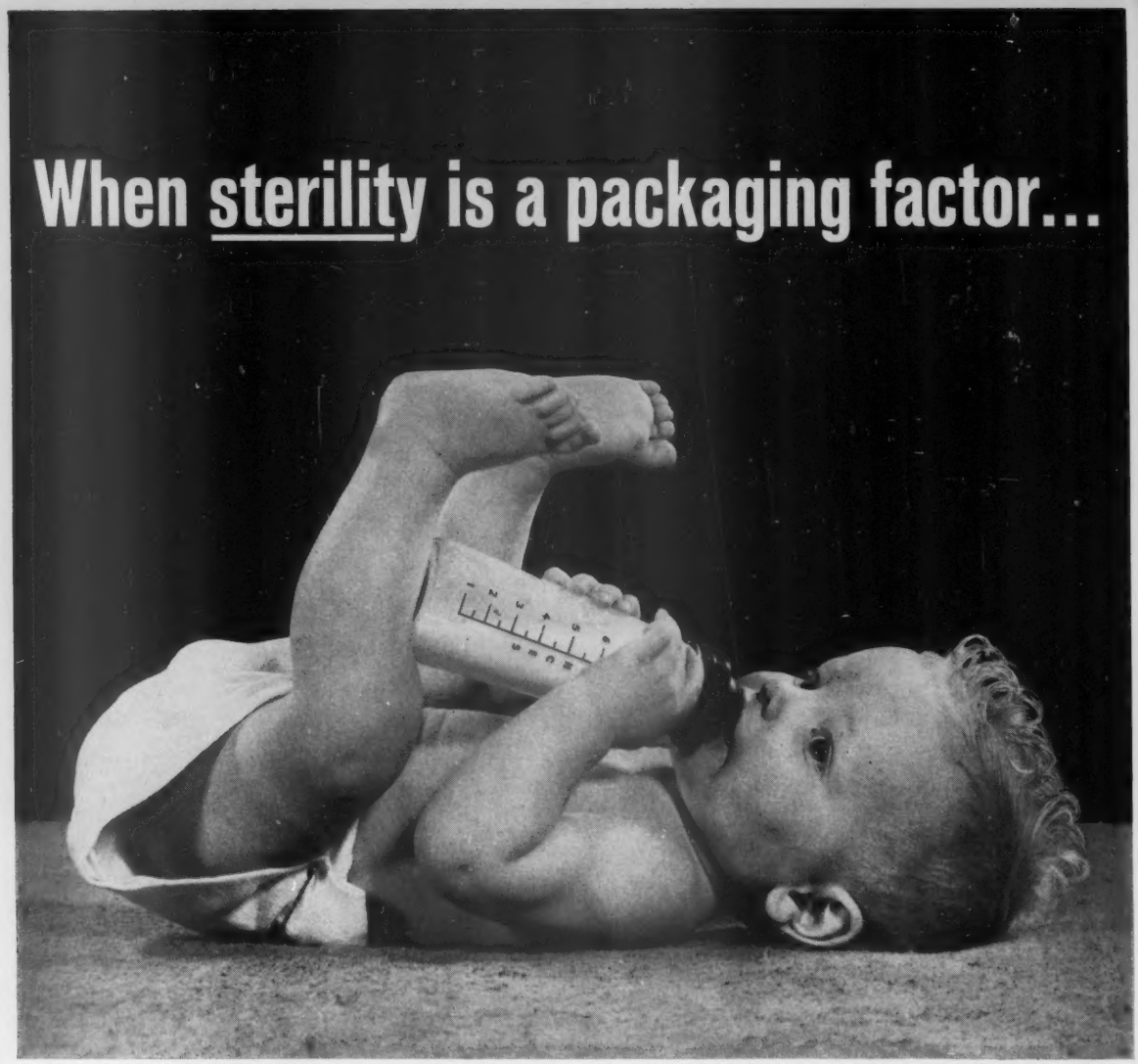

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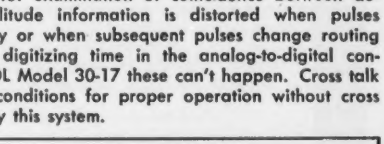

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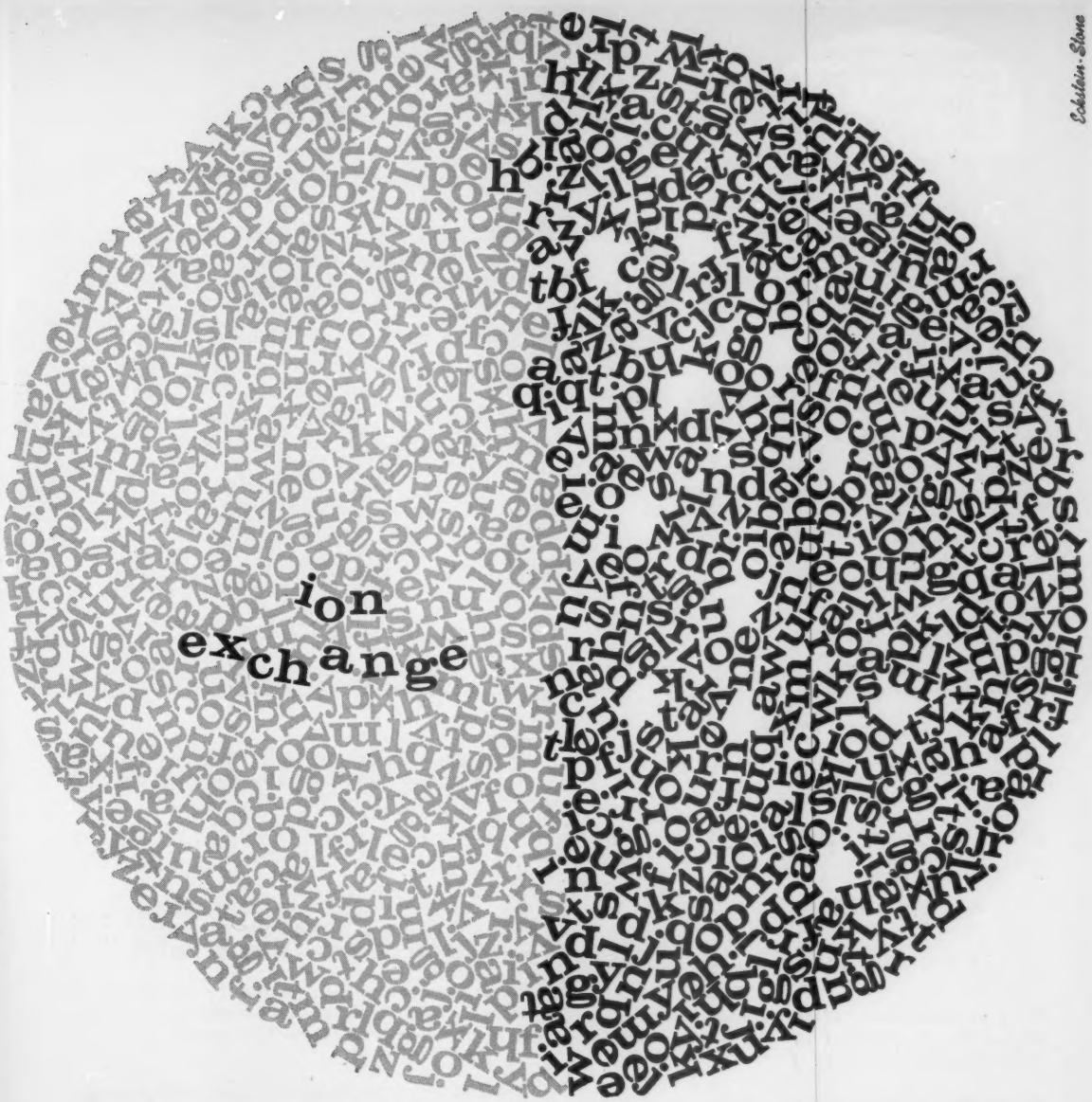

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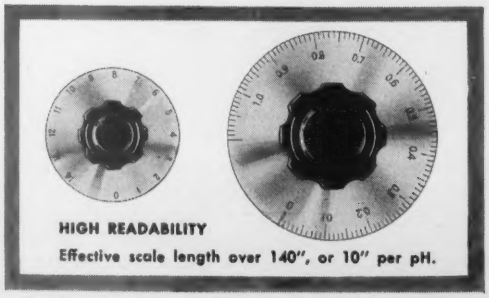

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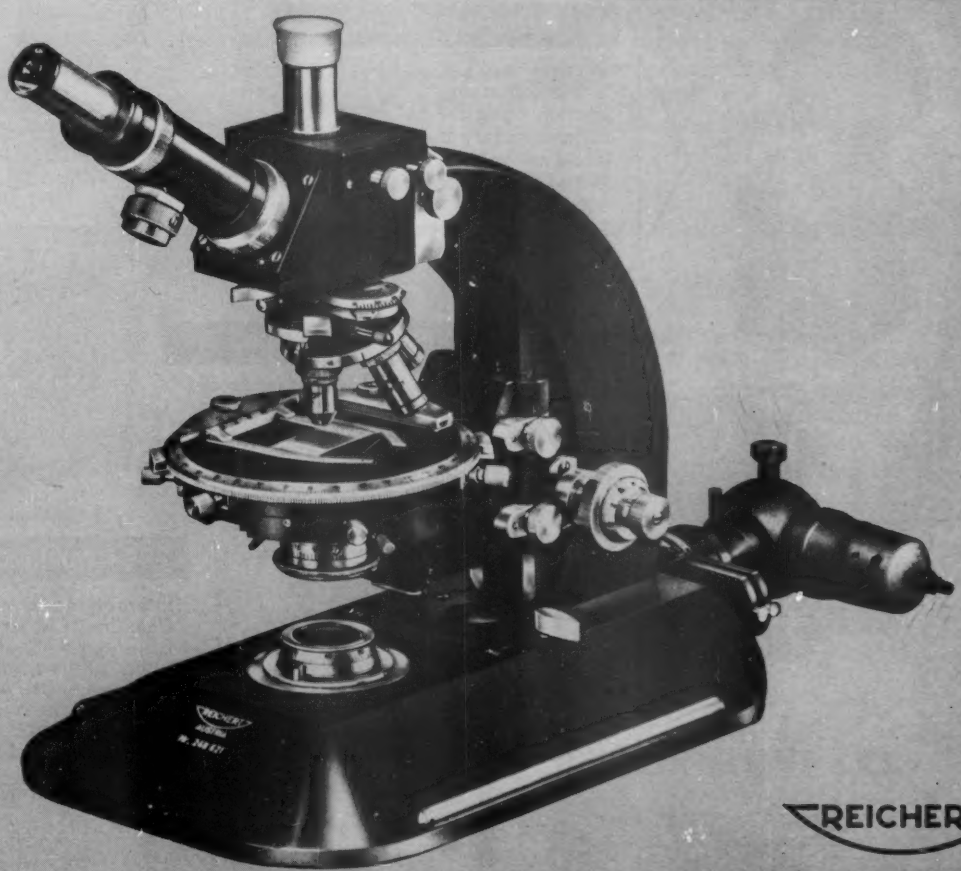

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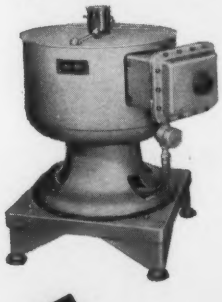

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International's Model PR-2 gives positive temperature control within 1°C to blood fractionations and similar separations between −20°C and +10°C. Twenty-eight interchangeable heads for capacities between 7ml and 4 liters provide versatility unmatched in the refrigerated centrifuge class.

All eight laboratory centrifuges displayed here bear the IEC trademark . . . the International symbol of optimum value. No other single manufacturer offers all eight. Yet, these trusted friends of thousands of laboratory directors and technicians are only the highlights of the world's most diversified family of fine centrifuges.

During 59 years of concentrated and progressive research on laboratory centrifuges, International has developed more models, more accessories, more special tooling than all other sources combined.

This specialized pool of knowledge and resources is available to you through an International-trained representative of your authorized International dealer. Whether your centrifuging problems are many or few, his unbiased advice can help you select the versatile or special-purpose International Centrifuge that fits your needs at lowest practical cost.

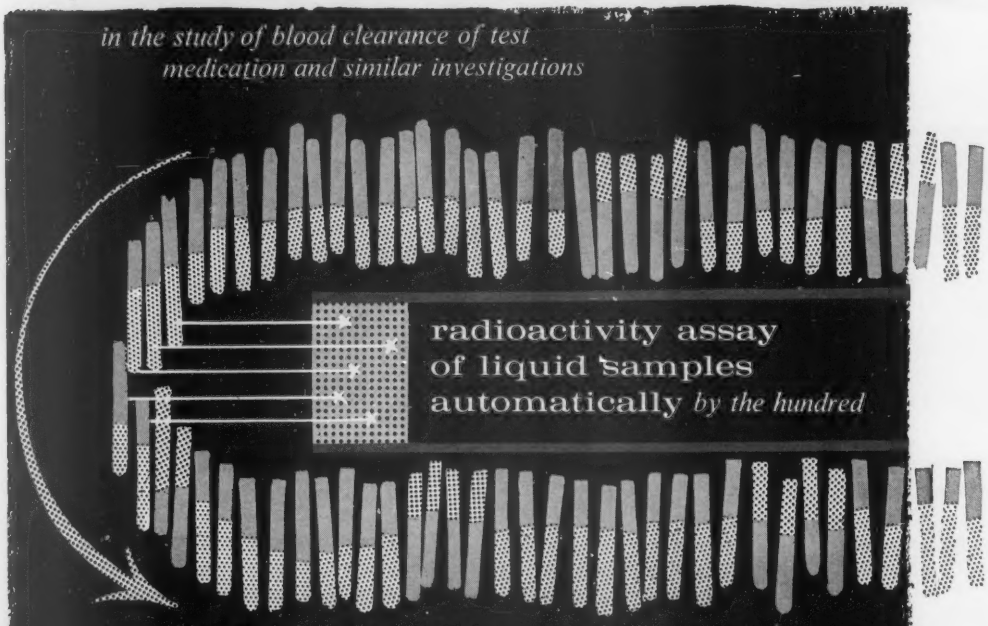
Before you choose your next laboratory centrifuge, get all the facts from your nearby International dealer or write:

INTERNATIONAL IEC EQUIPMENT CO.

1219 SOLDIERS FIELD ROAD, BOSTON 35, MASS.

ways PICKER NUCLEAR can help you...

in the study of blood clearance of test medication and similar investigations



Picker Nuclear can help you count *automatically* the gamma radiation from large numbers of liquid samples to free your technicians for more important work. Typical samples might be those from blood analyses conducted on a mice colony, from large scale physiology studies, from routine T-3 uptake measurements, etc.

The automatic system can handle 100 samples at a time. Test tube diameter may be 24 mm (for 15 cc samples) or 14 mm (for 5 cc samples). The results of the measurement may be recorded in *counts* and *time* (permitting preset limits on both) or in *elapsed time* (for preset counts). A pulse height analyzer can be included in the system to permit selective measurements of a single radioisotope in a mixture of isotopes.

This Automatic Well Counter is one of the comprehensive Picker line of nuclear instrumentation and supplies: all marketed and serviced through a national network of company offices staffed by trained Picker people. (Picker alone in the nuclear field offers this caliber of local service).

For details call your district office (see 'phone book) or write Picker X-Ray Corporation, 25 South Broadway, White Plains, New York.

Picker nuclear

The automatic counting set-up: the Picker Automatic Well Counter at left, next the Magnascaler and Count and Time Printer.

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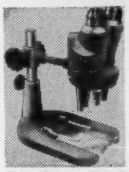

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Brand new Signal Corps Electric Generator for scientific experiments, electrical uses, demonstrations. Generates up to 90 volts by turning crank. Use in high impedance relays. Charge ground and bring up night crawlers for bait or study. 2 Alnico Magnets alone worth original price. Wt. 2 lbs. Cost to Govt. $15.

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Same type generator, mounted, with light, as electricity demonstrator.

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Photographers! This is an actual photograph of the moon taken through our Astronomical Telescope by a 17-year student.

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60 to 180 Power. An unusual Buy! Famous Mt. Palomar Type

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Projects illustrations up to 3" x 3½" and enlarges them to 35" x 30" if screen is 6½ ft. from projector; larger pictures if screen is further away. No film or negatives needed. Projects charts, diagrams, pictures, photos, lettering in full color or black-and-white. Operates on 115 volt, A.C. current. 6-ft. extension cord and plug included. Operates on 60 watt bulb, not included. Size 12" x 8" x 4½" wide. Weight 1 lb., 2 oz. Plastic case with built-in handle.

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Great idea! Fascinating fun and sometimes tremendously profitable! Tie a line to our 5-lb. Magnet—drop it overboard in bay, river, lake or ocean. Troll it along the bottom—your "treasure" haul can be outboard motors, anchors, fishing tackle, all kinds of metal valuables. 5-lb. Magnet is war surplus—Alnico V Type—a terrific bargain. Lifts over 125 lbs. on land—much greater weights under water. Order now and try this new sport.

Stock No. 70,183-W 5-lb. Magnet ----------$9.95 Postpaid

War Surplus American-Made 7x50 Binoculars

Big savings! Brand new! Crystal clear viewing—7 power. Every optical element is coated. An excellent night glass—the size recommended for satellite viewing. Individual eye focus. Exit pupil 7mm. Approx. field at 1,000 yds. is 376 ft. Carrying case included. American 7 x 50's normally cost $195. Our war surplus price saves you real money.

Stock No. 1533-W ----------Only $55.00 Postpd. (Tax included)

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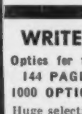

DE LUXE KIT for determining acidity and alkalinity, exisiting levels of nitrogen, phosphorus and potassium in the soil. Includes "Know Your Soil" dictionary, reagents and solutions to make 25 complete soil analyses (100 individual tests) in styrene 5" x 7" x 2" case.

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STANDARD KIT similar to above, with materials for 10 analyses and 15 additional pH tests. Comes in 5" x 7" x 1" plastic case.

Stock No. 70,406-W ----------$5.95 Postpaid

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The Vanguard Model 1000 VOLUMATIC Fraction Collector

The Vanguard VOLUMATIC is a completely transistorized self-contained unit employing an advanced technique for volumetric collection of chromatographic separations. Hold-up and mixing in volumetrically controlled separations are virtually eliminated when fractionation is performed with the Vanguard VOLUMATIC. Using a unique principle of repetitive cuts for a single separation, in conjunction with a photoelectric sensing device, the VOLUMATIC will collect from one to ten times the siphon volume in each test tube. The operator merely dials the number of times he wishes the siphon to fill and discharge before advancing to the next test tube. Employing this technique for collection of 5 X siphon volume for example, only the hold-up present from the last one-fifth of the first fraction is mixed with the first one-fifth of the second fraction, *an 80% reduction in mixing.*

Transistorization of all components assures absolute reliability of operation and allows continuous cold-room operation without modification.

The cast aluminum instrument cabinet affords the strength and rigidity needed for large columns and ancillary equipment, yet the entire unit weighs less than 50 lbs. Positive indexing of the stainless steel dispensing head to succeeding inner rows is achieved through mechanical gating which assures continued reliability. Compact size (25 in. wide x 30 in. long x 6 in. high) promotes maximum utilization of valuable laboratory and cold-room space. Heavy gauge, large capacity aluminum turntable (245 samples in 13mm. or 15mm. size) is supplied with handle and base-mounted rubber feet for easy removal and use as test tube tray.

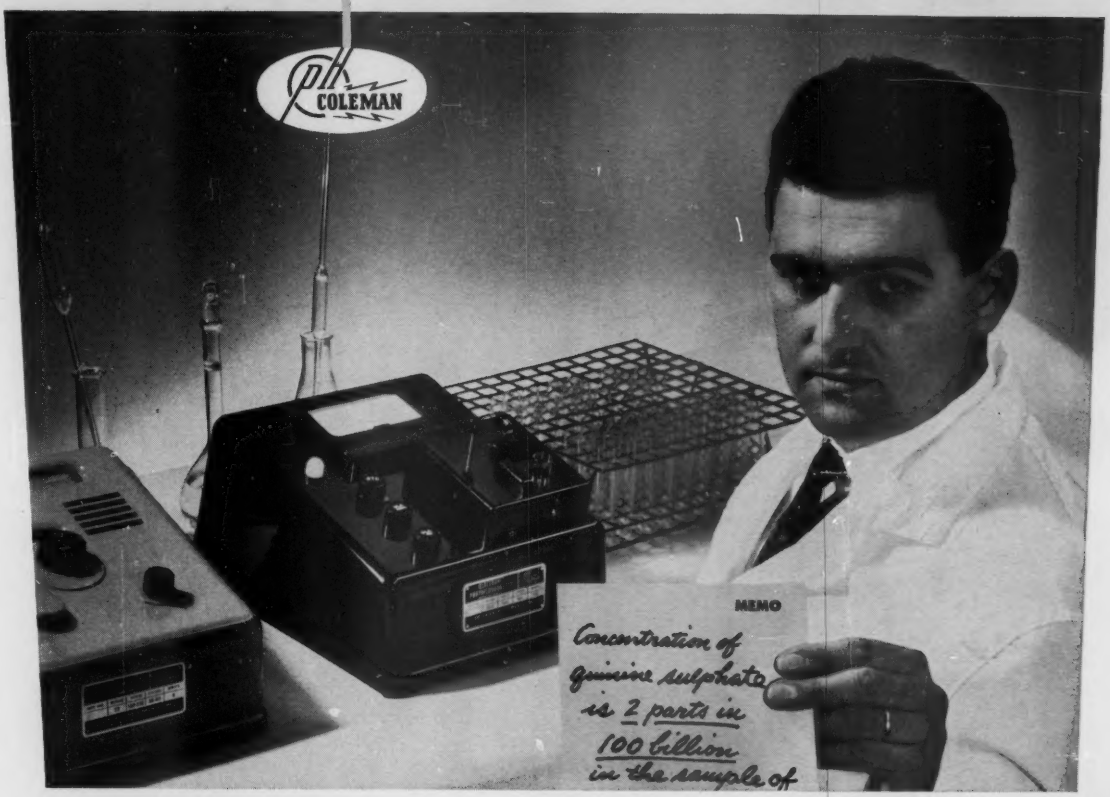
Interchangeable turntables for 13mm., 15mm. and 18mm. test tubes are offered as standard accessories. To meet varying requirements a complete selection of siphons is also available. To increase the versatility of the Vanguard VOLUMATIC, transistorized time and drop counting plug-in units are also available.

Complete unit including siphon and turntable of choice with 4 ft. column support rod priced at $695.00, F.O.B. LaGrange, Illinois.

P.O. Box 244
La Grange, Illinois
FLeetwood 4-5656

VANGUARD INSTRUMENT COMPANY

COLEMAN



The Coleman Photofluorometer® offers this surprising . . . and usable . . . sensitivity

It's true! The Coleman Photofluorometer® is sensitive enough to determine quinine concentration easily down to 3 parts in 10 billion—and when coupled with a Coleman Galv-O-Meter or Spectrophotometer, its sensitivity can be multiplied *15 times!* Determinations such as that shown above are *well* within its range.

This is one of the great advantages in using a Coleman Photofluorometer®—a tremendous sensitivity range without the use of photomultipliers.

In addition to high sensitivity, the Photofluorometer® remains unsurpassed for stability and ease of operation, with features like these:

Sample is placed directly in the optical system. No doors or carriers to handle.

Beam focuses within sample to eliminate interference by alien light or scratches and stains on the cuvette.

High intensity UV light plus continuous aperture control offer optimum excitation at all times.

Matched cuvettes save time and trouble.

Direct readings from large meter dial.

Eliminates need for expensive and troublesome photo-multiplier.

Decay of sensitive samples prevented by protecting sample from irradiation except during reading.

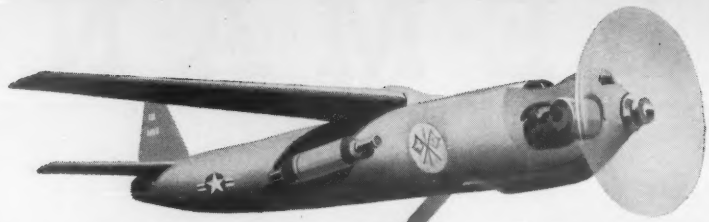
Filters are encased . . . protected from damage and many are pre-grouped for specific determinations.

Drift due to temperature change is prevented by the sample's position in the fan-cooled optical system.

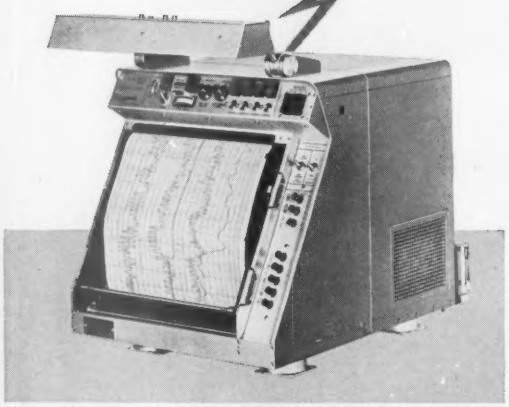
For complete data, ask for Bulletin SB-245A

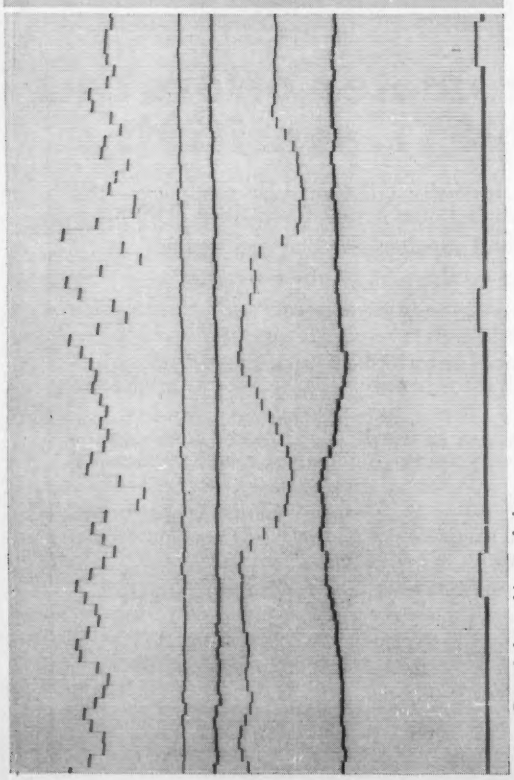
Order and simplification are the first steps toward mastery of any science

COLEMAN INSTRUMENTS, INC., MAYWOOD, ILLINOIS



Tracking a Surveillance Drone with the Visicorder





Record shown ¼ actual size.

Drone surveillance and reconnaissance gives U.S. Army combat units a high-altitude vantage point with much broader horizons from which to view battlefield action and terrain.

If effective use of the data gathered by the drone—the "eye in the sky"—is to be made, accurate instruments have to be on hand to monitor the drone's position and movement, its operational behavior and its response to flight commands. Telemetry supplies the radio link which transmits all this behavior information to a thoroughly-instrumented mobile tactical command post developed by Tele-Dynamics Division of American Bosch Arma Corp.

The Honeywell Model 1012 Visicorder has been selected as the direct readout unit in the Tele-Dynamics Drone Surveillance Telemetry system. In use with its companion instrumentation, the 36-channel Visicorder simultaneously displays the 22 channels of information required to track a drone, plus the timing traces.

In the Tele-Dynamics van, which serves as a tactical command post, the Visicorder provides both an instant "quick look" and a permanent record of the drone's operational parameters.

Signals are transmitted over a single channel by time-multiplexing. Signal and battery strength, engine speed and temperature, pitch and roll commands, altitude, airspeed, attitude (pitch and roll), yaw, acceleration (horizontal and vertical), and angle of attack are recorded by the Visicorder, along with three separate records of vibration.

Like the other units of the Tele-Dynamics system, these Honeywell Visicorders are built for rugged service . . . to deliver the data . . . when the drone is up and the chips are down.

Call your nearest Minneapolis-Honeywell Industrial Sales Office for a demonstration of how a Visicorder Oscillograph will save you time and money in data acquisition. OEM inquiries invited.

Reference Data: write for bulletins 906, 1012, 1108 and 1406.

Minneapolis-Honeywell Regulator Co.
Industrial Products Group, Heiland Division
5200 E. Evans Avenue, Denver 22, Colorado

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HONEYWELL INTERNATIONAL *Sales and Service offices in all principal cities of the world.*

NEW TORSION DIAL BALANCES with WEIGHT LOADERS

Torsion's new line of weight-loading dial balances retains all the proved advantages of the unique Torsion principle which eliminates knife edges and guarantees long-lasting accuracy.

Since Torsion introduced the "fine weighing" dial over a year ago, users have reported substantial savings in weighing time. Now Torsion has added a "weight-loading" dial which enables the user to "dial in" additional weights as described in the specifications for each new balance.

Both dials can be used without arresting the balance.

By using two dials, one for weight loading and one for fine weighing, Torsion has cut weighing time even more.

With Torsion's new two-dial feature, the time-consuming handling of small, loose weights has been eliminated. In addition to faster weighing, Torsion's new dial balances with weight loaders minimize the possibility of weights becoming inaccurate from rough handling.

Ask your laboratory supply salesman for a demonstration or write for complete specifications.

The **Torsion Balance** *Company*

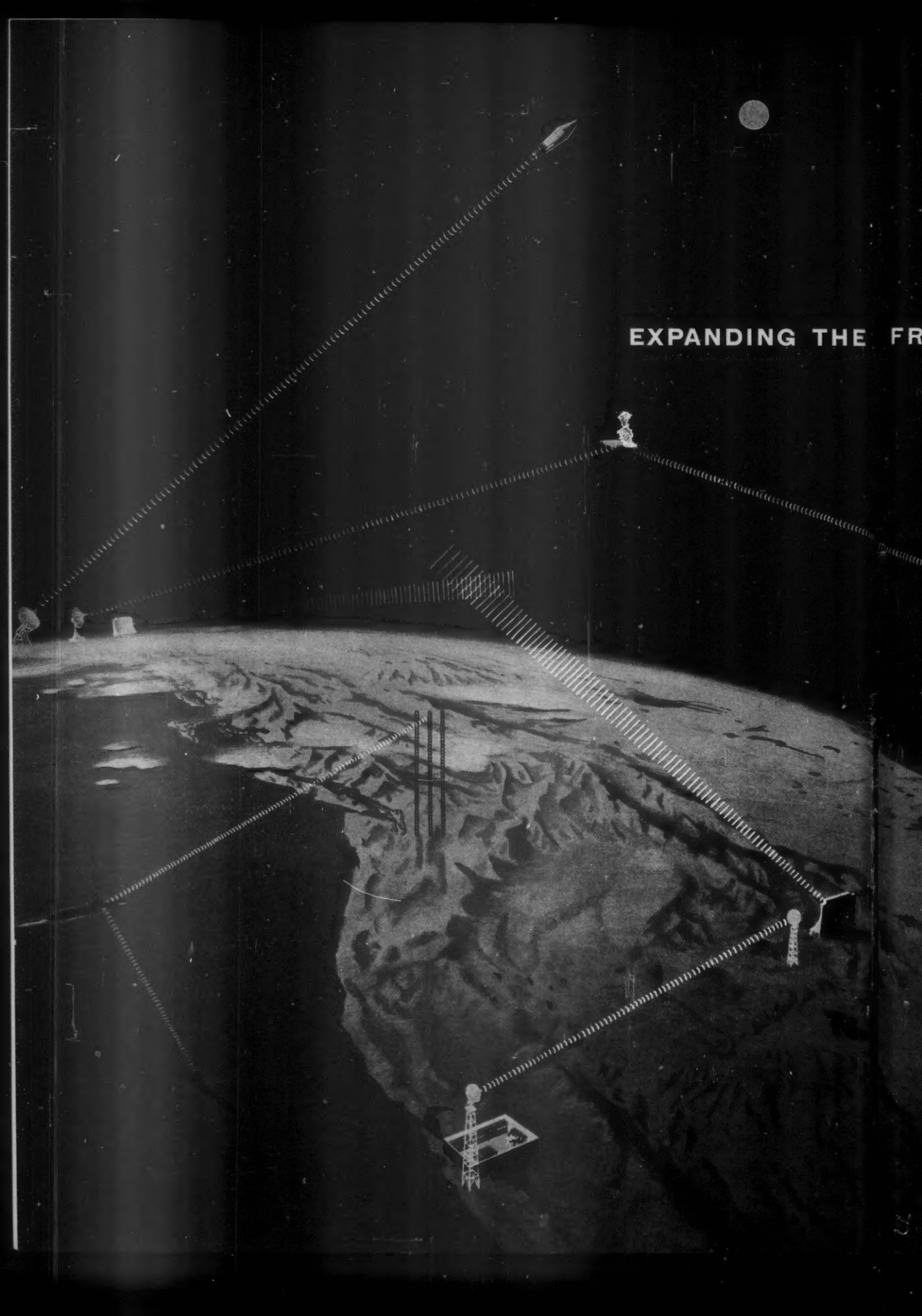
Main Office and Factory: Clifton, New Jersey
Sales Offices: Chi., Ill., San Mateo, Cal.

A TORSION MODEL DWL-3
Capacity: 200 grams
Weight-loading Dial: up to 9 grams by 1 gram increments
Fine Weighing Dial: 1 gram by .02 gram graduations
(Readability: .005 g)

B TORSION DWL-5
Capacity: 500 grams
Weight-loading Dial: up to 90 grams by 10 gram increments
Fine Weighing Dial: 10 grams by .1 gram graduations
(Readability: .02 g)

C TORSION DWL-2
Capacity: 120 grams
Weight-loading Dial: up to 9 grams by 1 gram increments
Fine Weighing Dial: 1 gram by .01 gram graduations
(Readability: .002 g)

TORSION DWL2-1
Specifications are same as the DWL-2 except that this model has scoop for seeds or other bulky material.


EXPANDING THE FR

Herodotus, the historian, records (490 B.C.) the use of burnished shields for military signaling. This was the forerunner of the heliograph, invented by Sir Henry C. Mance, which came into wide use centuries later.

E FRONTIERS **OF SPACE TECHNOLOGY IN**

COMMUNICATIONS

Lockheed's interest in developing the science of communications extends from the depths of the oceans to deep space. Its Missiles and Space Division research programs deal with the development and application of statistical communication and decision theory in such areas as countermeasures; telemetry multiplexing and modulation; scatter communications; multiple vehicle tracking; millimeter wave generation and utilization; sonic signal detection and processing; avoidance of multipath degradation; and interference avoidance.

Associated research and development efforts are directed toward propagation studies and advanced antenna design; low noise amplifiers; vehicle borne signal transmission and reception, data storage and processing; solid state materials and devices.

The scope of such activities extends from advanced studies of naval communication problems on and under the oceans; the many applications to satellite vehicles; on to the specialized communication problems of deep space explorations. Latter needs are exemplified by high frequencies, low weight and power, high stability, low effective bandwidth, extreme reliability and basic simplicity requirements.

Engineers and Scientists: Investigating the entire spectrum of communications is typical of Lockheed Missiles and Space Division's broad diversification. The Division possesses complete capability in more than 40 areas of science and technology – from concept to operation. Its programs provide a fascinating challenge to creative engineers and scientists. They include: celestial mechanics; communications; computer research and development; electromagnetic wave propagation and radiation; electronics; the flight sciences; human engineering; magnetohydrodynamics; man in space; materials and processes; applied mathematics; oceanography; operations research and analysis; ionic, nuclear and plasma propulsion and exotic fuels; sonics; space medicine; space navigation; and space physics.

If you are experienced in work related to any of the above areas, you are invited to inquire into the interesting programs being conducted and planned at Lockheed. Write: Research and Development Staff, Dept. M-18A, 962 W. El Camino Real, Sunnyvale, California. U.S. citizenship or existing Department of Defense industrial security clearance required.

Lockheed / **MISSILES AND SPACE DIVISION**

Systems Manager for the Navy POLARIS FBM and the Air Force AGENA Satellite in the DISCOVERER and MIDAS Programs

SUNNYVALE, PALO ALTO, VAN NUYS, SANTA CRUZ, SANTA MARIA, CALIFORNIA
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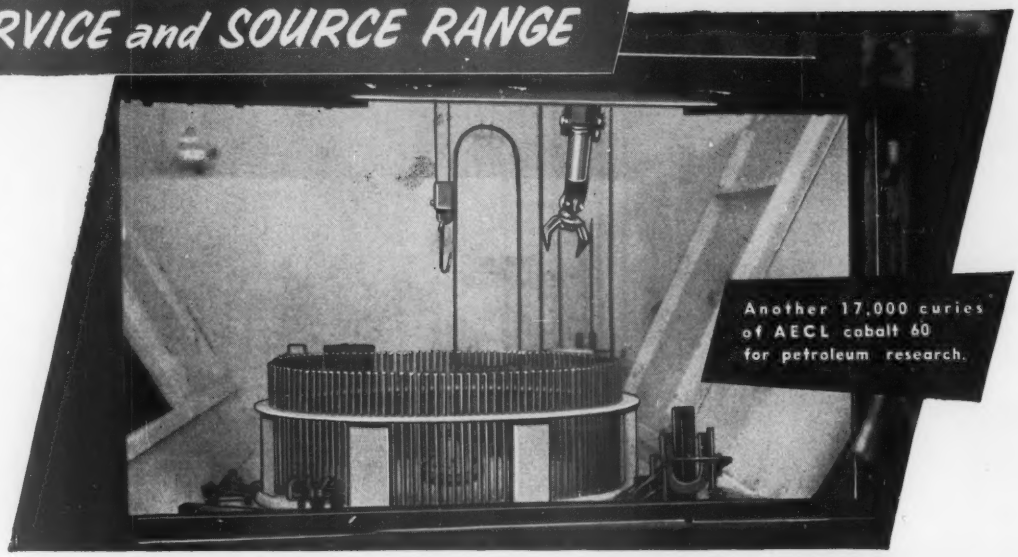
COBALT 60

SERVICE and SOURCE RANGE



A "Weldcaps" Cobalt 60 source supplied by AECL, in the hot cell at SINCLAIR RESEARCH LABORATORIES INC., Harvey, Illinois.

For your Gamma Irradiation requirements choose A.E.C.L. COBALT 60

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- Penetrating — (excellent uniformity of dose),
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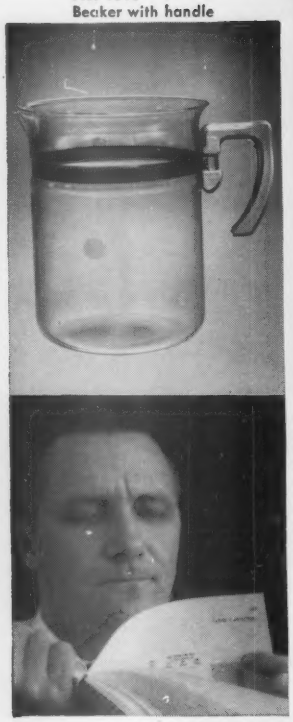
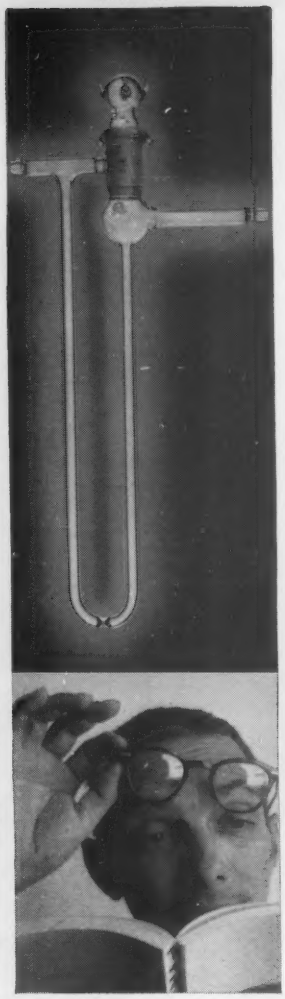
Cut-Out And Paste On Post Card

No. 1010
Beaker with handle



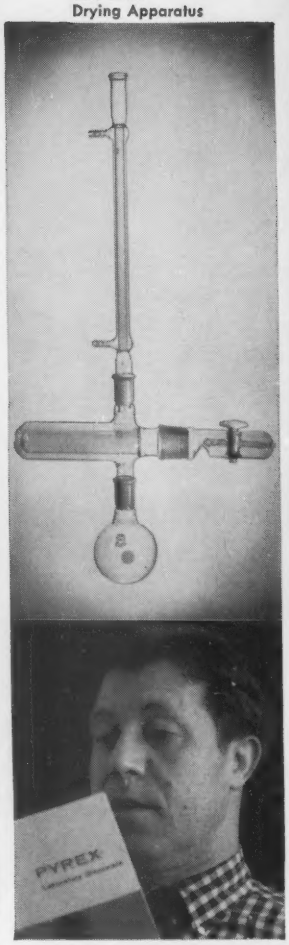
Hmm, a beaker with a handle. Makes sense. Should lift and pour as easy as a coffeepot – even when it's boiling hot. Won't need those clumsy tongs now.*

No. 5960
Flowmeter



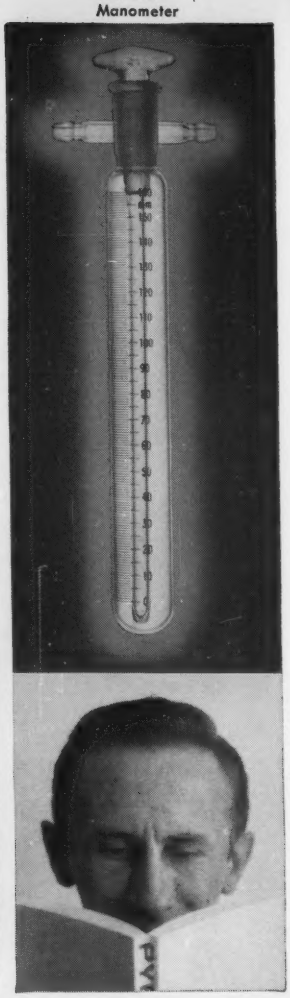
Four orifices on this flowmeter. From ¼ to 2 mm. Says you just turn the stopper to select the one you want. That's pretty simple.*

No. 3690
Drying Apparatus



Drying apparatus for small quantities. Works at constant temperature, under reduced pressure. I won't have to buy separate parts and make my own after all.*

No. 6952
Manometer



A "t" manometer. Scaled from 0 to 160 mm. The scale is red for easy reading. Available in single outlet also. Real easy to fill.*

* MORAL?

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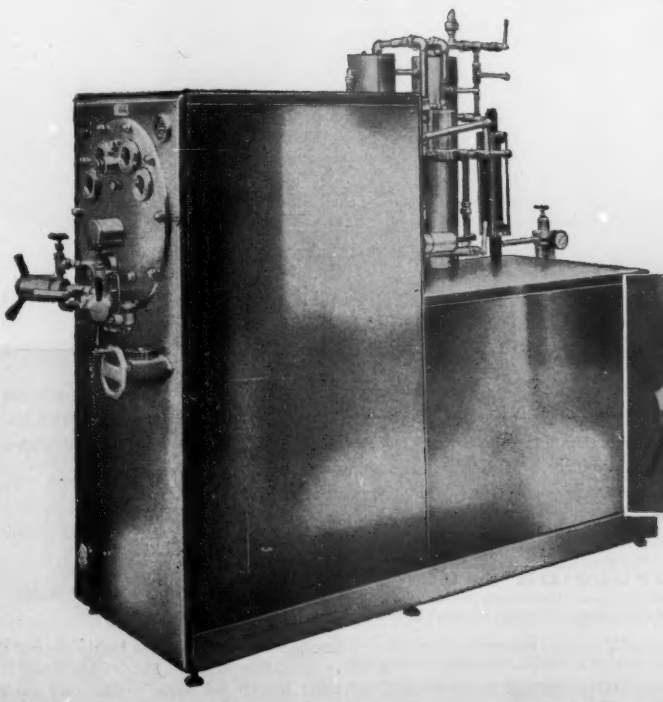

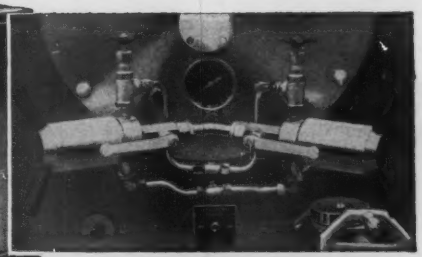

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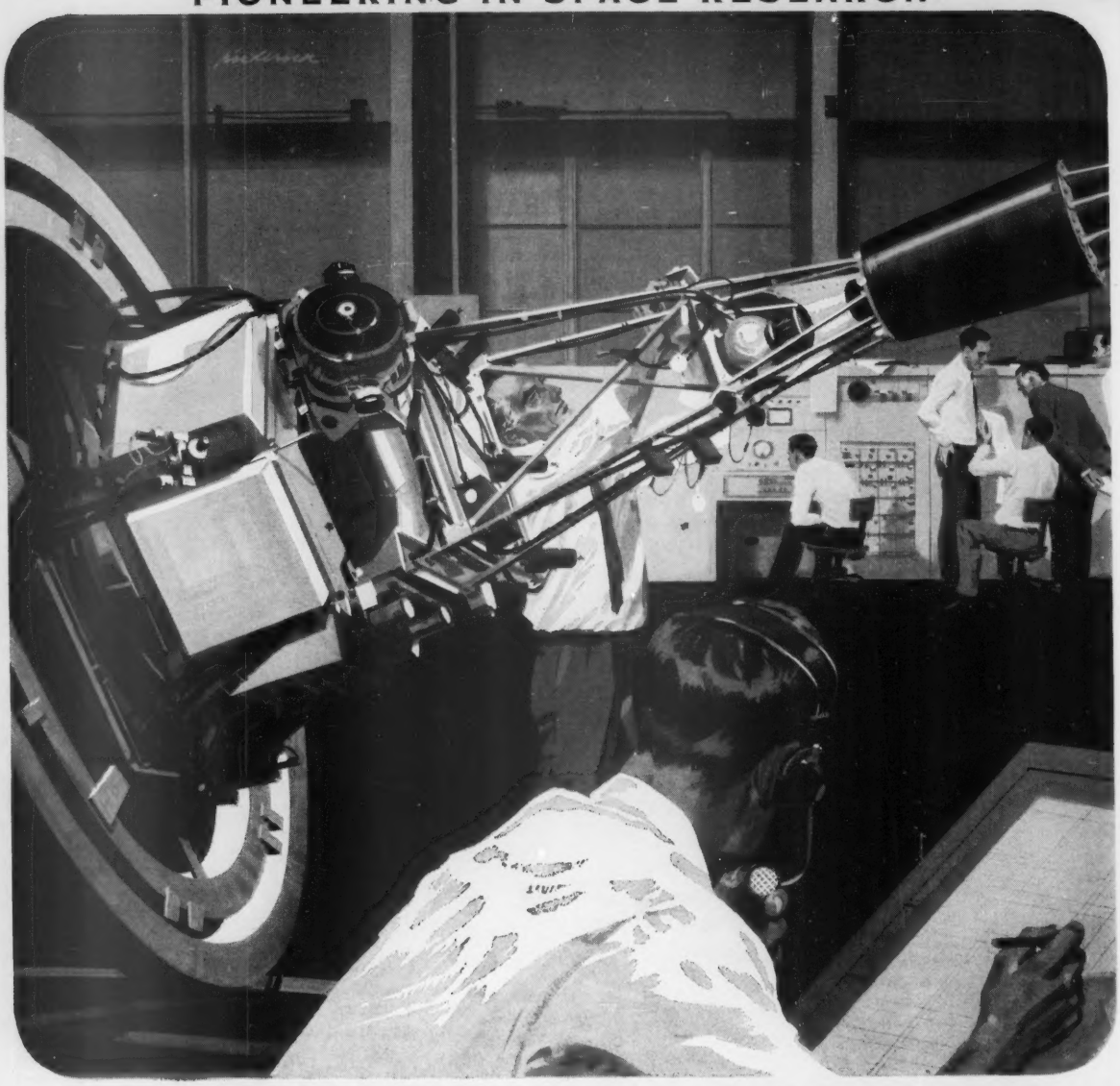

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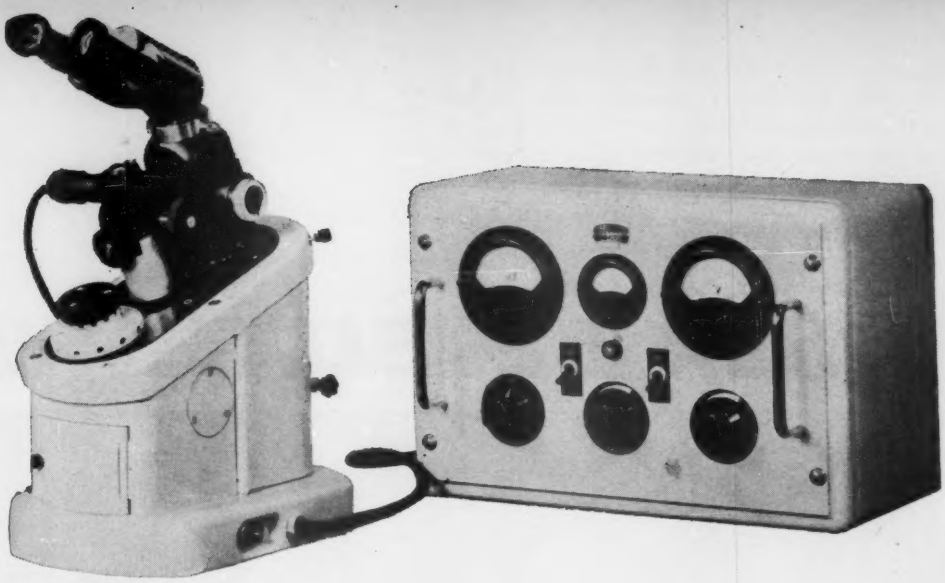

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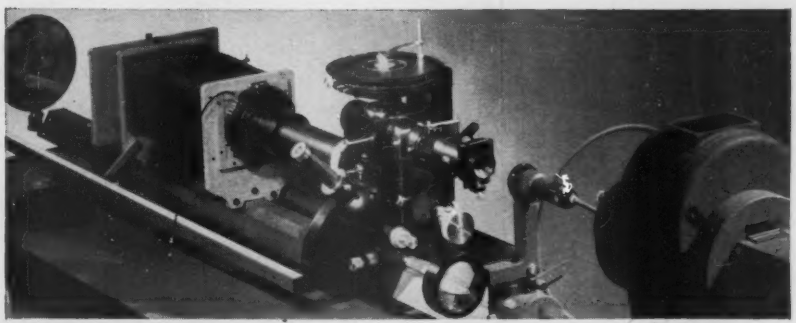

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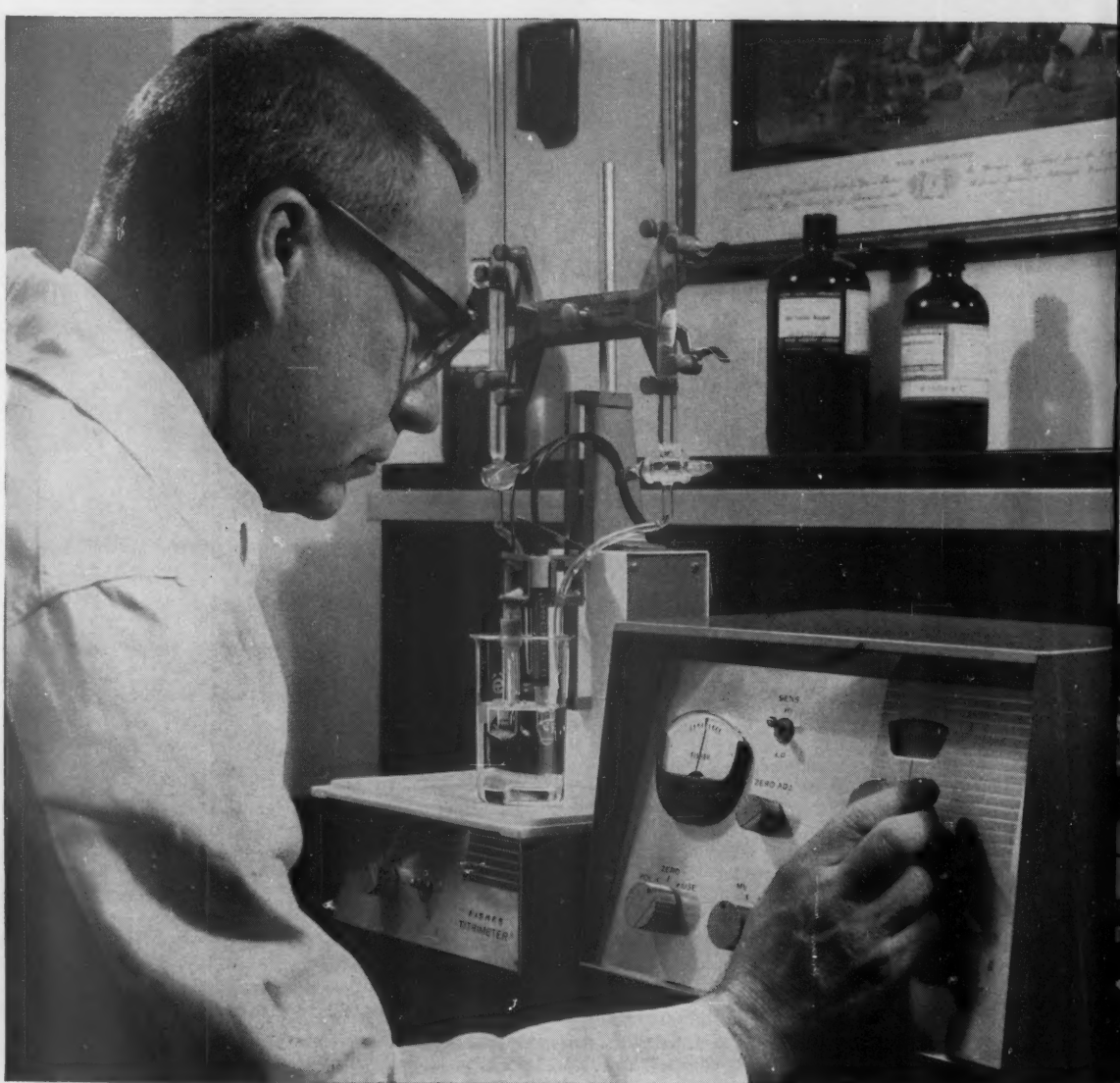

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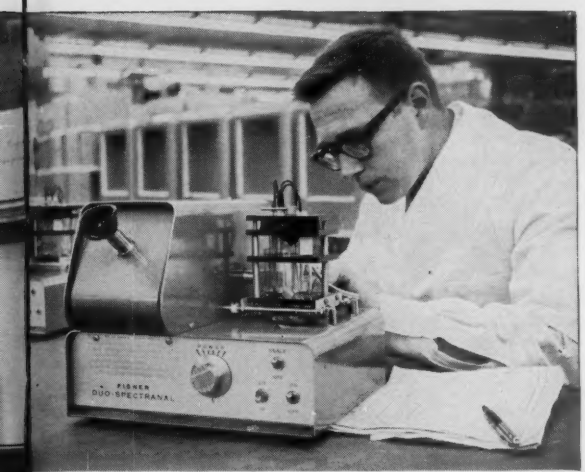

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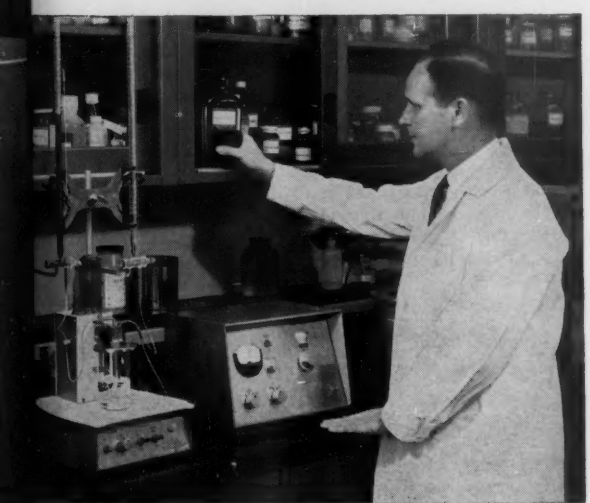

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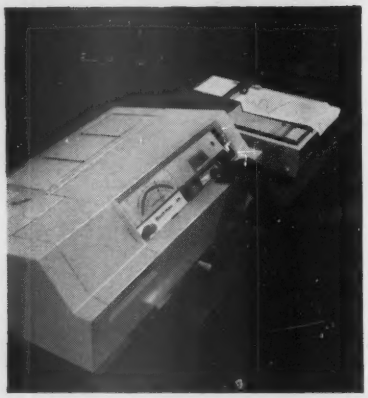

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Letters

Conservation

Thank you for your cover picture [*Science* **132** (9 Dec. 1960)] calling attention to the book, *This Is the American Earth*, one of the Sierra Club's most important achievements in 68 years of conservation. We are happy that your reviewer, ecologist Edward S. Deevey, author of one of the liveliest essays I have ever read ("The hare and the haruspex," in the *Yale Review*), liked the pictures [**132**, 1759 (1960)]. Other ecologists we have heard from liked the rest of the book as well.

Nancy Newhall chose a method of presentation that she hoped would jar readers out of the Conventional Response. The result may not be comfortable, but then it wasn't intended to be. As one who has watched the response to various kinds of conservation writing for more than 20 years, I think her choice, in its organic beauty, warrants the high praise that it has received elsewhere. Not Deevey's ridicule. He sticks labels on the conservation effort that will be much harder to scrape off than they were to put on.

Before undertaking a review that could severely impair the reception accorded a book by so important an audience as yours, I think a reviewer should read the book carefully, not just skim it. There is abundant evidence of skimming in the review, patent in the fact that a man as brilliant as Deevey missed the point of the book as no other reviewer has. In implying that Nancy Newhall's text is concerned only with scenic resources and not with broad conservation, he misses her thesis by a mile. The text is about the survival of man. It concerns the relation of man's spirit—the crucial resource—to his environment, on which he must live much more lightly than he has been doing. An ecologist, of all people, must know how totally inadequate the shallow definition of conservation ("wise use") is in the mid-20th century—even when qualified to read "ecologically sound use."

This Is the American Earth is not a book to be read in smug assurance that science has solved everything, or soon will have. It tells no one to relax and enjoy man's present course; it tries to change that course to one with good promise for our children. "Conservation is humanity fighting for the future," Nancy Newhall writes. It is not a methodological gathering of data indicating a need for further study about the rate of expenditure of resources.

Nor is conservation served by desiccating emotion out of its literature. One of the needs in conservation is the ability to express deep-felt opinion, to compress considerable scientific fact into poetic form, and especially to stir people into caring enough to act, and to act in time.

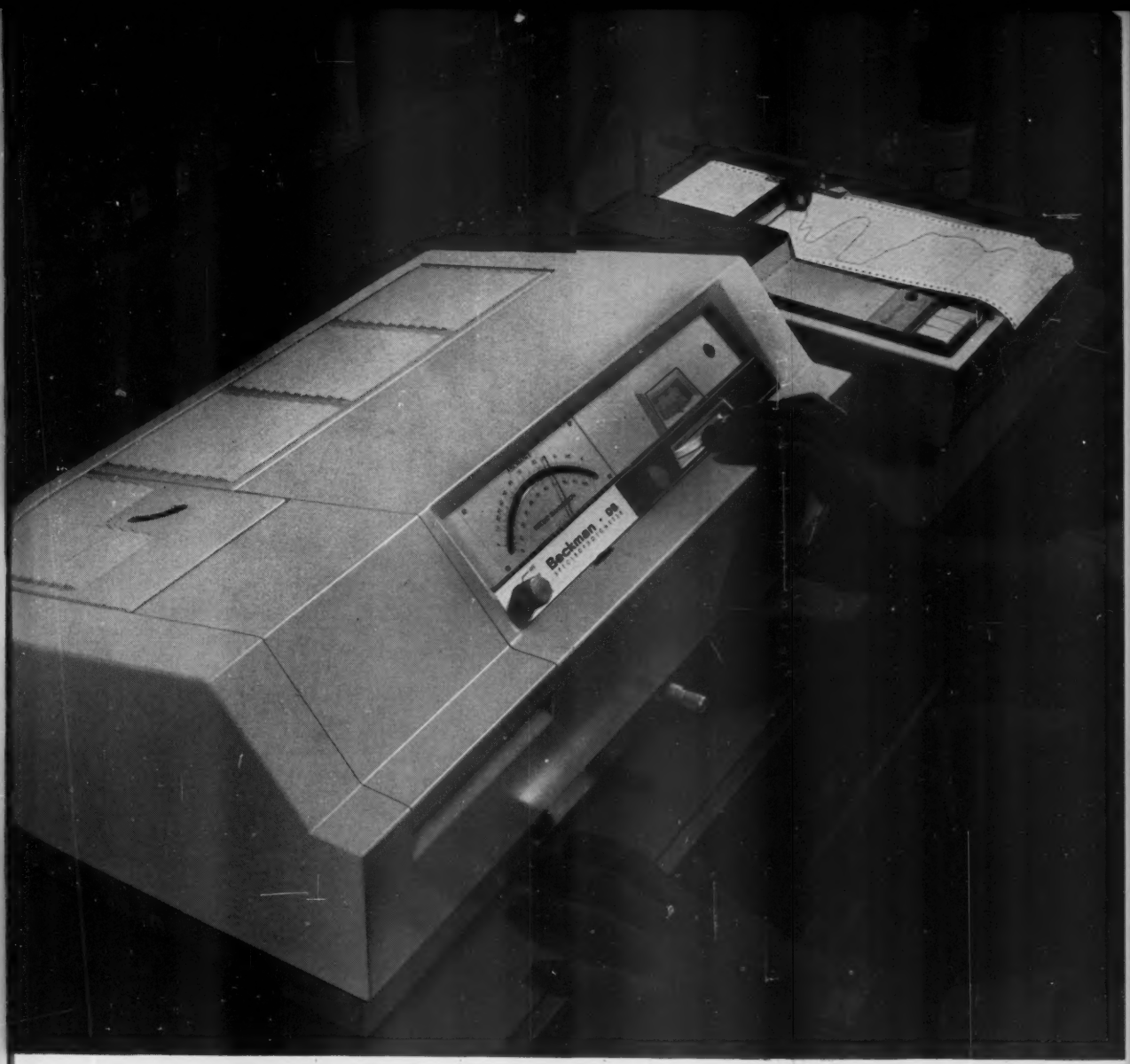
We wish your readers would check for themselves what Nancy Newhall, as an artist and writer lauded by artists and writers (if not by one ecologist) and as a conservationist and sciolist if not as a scientist, has been able to do. We think they will conclude it was worth doing. (I can't resist adding, about the text that Deevey didn't like, that Alfred Knopf, no mean connoisseur of the written word, has proposed publishing the text *without* the photographs!)

DAVID BROWER
Sierra Club, San Francisco, California

I am glad, with David Brower, that many reviewers liked Nancy Newhall's text, but I would be more penitent about being caught in a minority position if I were sure that the reviewers are not confusing their own commitment to the cause of conservation with literary discernment. In my review, which certainly was ungracious, I tried to nail an attitude that is wholly natural to movements of social reform, but which seems to me to pose a grave internal danger: the tendency of partisans to talk only to each other, in a private language that is bound to be misconstrued when overheard. I think it is poor tactics to broadcast this language. I quite agree that conservation is "humanity fighting for the future," but I suspect that field commanders like Brower, to whom we must all be grateful as they conduct our battles for us, cannot see all sectors of the front at once; and I think the movement is strong enough to stand some frank discussion, intramurally and behind the firing lines, of its methodology and goals.

To deal first with tactics, let me make plain that I ridiculed neither the book nor the movement, nor do I oppose emotion or art in the service of policy. What I said was that effusive overstatement is a dangerous political weapon, since its users can be made to *look* ridiculous. We agree, *entre nous*, that the book is propaganda—*Paradise Lost* was propaganda too. Now emotion is the very stuff of art, but when art is used as propaganda one wants to be quite sure that the emotion not only is honest but *sounds* honest—otherwise the irreverent opposition labels it sentimentality, meretriciousness, or cant. And it takes very great literary skill to express honest emotion nobly enough to spike such charges; sincerity is necessary but insufficient. I do not question Nancy Newhall's sincerity. I do find

(Continued on page 922)



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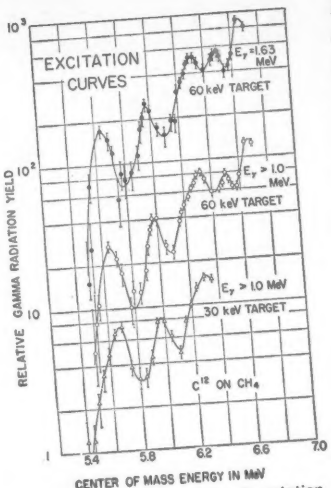

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A paper at our recent Accelerator Conference, "Current Experimentation with the Tandem Accelerator at the Chalk River Laboratories[1]," describes an outstanding experimental physics program. Write us for a copy.

"Low-Energy" Physics

As we address ourselves to this subject, more elegantly called *nuclear-structure physics*, the reader may conclude we have an axe to grind, and we admit it. We believe a great deal of research remains to be done on light nuclei. There is, for example, time-consuming but rewarding precision nuclear spectroscopy to fill in gaps in existing energy level data, as well as new research related to the conservation of isotopic spin, excitation energies of low excited states and direct interaction mechanisms.

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[1]*H. E. Gove, Proceedings of the Second Accelerator Conference, Amsterdam, Oct., 1960 (North Holland Publishing Company, 1961) p. 63.*

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Twenty-four hours a day, seven days a week, the Voice of America broadcasts radio programs in 35 languages from 87 transmitting stations. Many of the programs are available on tape for foreign distribution and rebroadcast, and many are recorded when received by foreign stations and rebroadcast later.

In addition to regular weekly reports on science and medicine in the United States, the Voice of America has during the last two years produced several series of programs about science and scientists: Frontiers of Knowledge; International Geophysical Year Series; American Men of Science; Science Features; Science in the News; Birth of a Spaceman; The Ocean Depths—A New Frontier; Man in Space—Project Mercury; Atoms for Power; The New World of Atomic Energy; and Forum—The Arts and Sciences in Mid-Century America.

The Forum series is of special interest in that it makes no attempt to reach a mass audience: it is directed to intellectuals abroad. In the sciences, two series of Forum lectures were given in 1959: a 16-program series in medicine and a 20-program series in the behavioral sciences. In 1960 an 11-program series in chemistry was produced, and the first of 20 lectures in the biological sciences was broadcast last week. A few titles from the latest series and the names of the lecturers give some idea of the scope and quality of the programs. In chemistry, the following are representative: Chemical Research in Solar Energy, Farrington Daniels; Radioactive Isotopes in Chemical Research, Paul C. Aebersold; Proteins, John T. Edsall; and Origin of the Solar System, Harrison Brown. In biology some sample lectures are: Characteristics of Animal Populations, Edward S. Deevey; Plant Photoperiods, H. A. Borthwick; Biochemistry of Human Heredity, H. Bentley Glass; Chromosomes and Tissue Culture, Theodore T. Puck; Enzyme Feedback Controls of Living Processes, DeWitt Stetten.

The lectures in this series are available abroad not only on tape but also in the form of pamphlets which are obtainable for the asking at any of the 200 foreign posts of the United States Information Service or by direct request to the Voice of America in Washington.

Some Americans who are aware that these Forum lectures may be freely published or broadcast overseas have been under the impression that they cannot be used in a similar way in the United States. This is in part true, in that it is easier for a foreigner to obtain and publish the material: all he needs is a copy or a tape. A domestic user must in the first place get wind of the lectures and then get permission of the lecturers to use the material. Subject to the lecturers' permission. commercial publishers are, however, planning to bring out at least three of the Forum series in paperback editions, and the National Association of Educational Broadcasters will make the tapes available for further distribution. Thus it will soon be possible for Americans to have access to much of the material that is now virtually limited to a foreign audience. They then can judge for themselves whether an English chemist was justified in writing recently that these lectures would assist him and his colleagues in keeping up with modern developments in science.—G.DuS.

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CURRENT PROBLEMS IN RESEARCH

The Planet Venus

Recent observations shed light on the atmosphere, surface, and possible biology of the nearest planet.

Carl Sagan

The launching of the Soviet interplanetary vehicle toward Venus on 12 February 1961 opens a new era in planetary studies. This article is an assessment of current knowledge of Venus at the dawn of this era.

The planet Venus is enshrouded by clouds which prevent telescopic examination of its surface. In the absence of direct observations, reasons have been adduced for proposing a variety of differing and mutually inconsistent surface conditions. Since only water clouds were familiar to terrestrial observers, the apparent thickness of the Cytherean (*1*) cloud layer seemed to argue for a great abundance of water. From there it was only a step to the assertion (*2*) that "everything on Venus is dripping wet. . . . A very great part of the surface of Venus is no doubt covered with swamps. . . . The constantly uniform climatic conditions which exist everywhere result in an entire absence of adaptation to changing exterior conditions. Only low forms of life are therefore represented, mostly no doubt, belonging to the vegetable kingdom; and the organisms are nearly of the same kind all over the planet."

After many unsuccessful spectroscopic attempts to discover water vapor in the Cytherean atmosphere, the hypothetical Carboniferous swamp was generally abandoned, to be replaced by an arid planetary desert, overlain by clouds of dust from the wind-swept surface (*3*) (Fig. 1). The arid surface also explained the great abundance of carbon dioxide [which was accidentally discovered (*4*) in a search for water vapor]; for, in the absence of water, the Urey equilibrium pressure of carbon dioxide will not be established (*5*). Hoyle (*6*) explained the lack of water by assuming a great excess of hydrocarbons over water on primitive Venus, and subsequent oxidation of the hydrocarbons to carbon dioxide, until all the water was depleted. He suggested that the surface is now covered with the remainder of the hydrocarbons, and that the cloud layer is composed of smog.

Menzel and Whipple (*7*) replaced the wind-swept desert and the planetary oil field with a global Seltzer ocean; they argued that if Venus were completely covered by water (because of the high atmospheric content of carbon dioxide, the water would, of course, be carbonated), the access of carbon dioxide to silicates would be impaired, and for this reason the Urey equilibrium would not be established. The state of our knowledge of Venus is amply illustrated by the fact that the Carboniferous swamp, the wind-swept desert, the planetary oil field, and the global Seltzer ocean each have their serious proponents, and those planning eventual manned expeditions to Venus must be exceedingly perplexed over whether to send along a paleobotanist, a mineralogist, a petroleum geologist, or a deep-sea diver. But new information has recently become available which probably eliminates three of the four proposed surface environments; taken together with some of the earlier data, it points the way to a consistent picture of the atmosphere and surface of Venus.

Composition of the Atmosphere

Only the portions of the Cytherean atmosphere which are above the cloud layer are accessible to spectroscopic investigation. Since the cloud layer may be situated tens of kilometers above the surface (see the discussion below), the spectroscopic data are not necessarily directly applicable to the lower atmosphere. It is possible that gases present above the cloud layer in undetectable amounts are abundant in the lower atmosphere. By laboratory intensity-matching of the Cytherean carbon dioxide intercombination bands near 8000 angstroms, the abundance of carbon dioxide (above the atmospheric level at which an 8000-angstrom photon is effectively reflected) is estimated to be 1 kilometer-atmosphere (km-atm) (*8*). The only other possibly identified atmospheric constituent is water vapor, marginally detected on Venus recently by high-altitude balloon spectroscopy. The abundance of water vapor (above the atmospheric level at which a 1.13-micron photon is effectively reflected) is estimated by Strong to be about 2×10^{-3} gm/cm^2 (*9*). Kozyrev reports observing several features in the aurora and night sky of Venus corresponding in wavelength to known emission bands of N_2, N_2^+, and CO^+ (*10*). From considerations of cosmic abundance and from terrestrial analogy, we would expect to find N_2—which has no permitted absorption spectrum in the presently accessible wavelength region—on Venus, and we would expect to

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find carbon monoxide from the photodissociation of carbon dioxide. [It is a curious incidental fact that at one time Kozyrev (*11*) believed the spectral features discovered by him were due to microorganisms in the Cytherean atmosphere.] Nevertheless, Newkirk (*12*) found no evidence of many of the strongest features reported by Kozyrev and found strong emission at a neighboring wavelength (4505 A) not reported by Kozyrev and corresponding to no known molecular emission. The only features common to both observations are unidentified emission bands at 4415 and 4435 angstroms (*13*).

One might attribute the absence of, for example, the N_2^+ first negative system in Newkirk's spectra to low magnetic activity at the time of his observations, but the absence of the feature at 4505 angstroms in Kozyrev's spectra then remains puzzling. Unsuccessful searches for carbon monoxide absorption place the amount of carbon monoxide above the relevant reflection level at less than 100 cm-atm (*14*). At the present writing, it must be concluded that there is no convincing direct evidence for nitrogen or carbon monoxide on Venus. The present upper limit on oxygen in the high Cytherean atmosphere is 100 cm-atm (*3*, *15*); the limit is 100 cm-atm for N_2O, 4 cm-atm for NH_3, 20 cm-atm for CH_4, 3 cm-atm for C_2H_4, and 1 cm-atm for C_2H_6 (*14*). None of these gases has been identified. The last few abundances argue effectively against the hypothesis that the cloud layer is composed of hydrocarbons (*6*, *16*); at the observed temperature of the cloud layer, the vapor pressures of gaseous hydrocarbons are greater by orders of magnitude than the spectroscopic upper limits.

Nature of the Cloud Layer

Visual observations of Venus generally show very little detail, owing to the uniformly high albedo of the cloud layer. During daylight telescopic observations, when comparisons with terrestrial clouds can be made, it is evident that the planet is a pale lemon yellow. The yellow coloration of Venus is also observed photometrically (*17*). Under the best seeing conditions, faint dark markings can be perceived on the illuminated part of the disk. A broad band of shade adjoining the terminator sometimes appears brown (*18*). Other markings, both bright and dark, can be observed; occasionally dark bands are seen extending perpendicularly from the terminator onto the disk. The difficulties encountered in making visual observations of Venus are illustrated by the fact that the outstanding American observer at the turn of the century, in over a decade of regular observation of Venus, was able to see distinct markings only once. On that occasion shading was evident, parallel to the terminator but not perpendicular to it (*19*). Danjon and Dollfus (*20*) have constructed planispheres from their visual observations which show dark markings with little relative displacement over periods of weeks. In some drawings the markings tend to radiate from the subsolar point. Danjon and Dollfus interpret these dark markings as surface features seen through stable gaps in the cloud layer. The apparent constancy in the position of the markings suggested to the French astronomers that the period of rotation of Venus is equal to its period of revolution, 225 days. However, Kuiper (*21*) has pointed out that on a slowly rotating planet, cloud patterns near the terminator will bear an approximately constant relation to the terminator, just as characteristic cloud patterns appear at a given time of day on the earth.

Photographic detail of Venus is most evident in the near-ultraviolet region and least evident in the infrared. It is impossible to explain this circumstance by an inverse-power scattering law and the hypothesis that the observed detail is below the visible cloud layer. Detail on Mars becomes more evident toward the longer wavelengths. It is tempting to suggest the presence around Venus of high-altitude clouds which are opaque to ultra-violet light and more transparent in the infrared. An alternative explanation is that the cloud layer is billowy, so that low-lying clouds appear darkened in the violet because of the increased scattering of light by the overlying atmosphere (*14*). The classic ultraviolet photographs by Ross (*18*) (Fig. 2) show time-variable bright and dark areas, sometimes stretching, band-like, perpendicularly from the terminator onto the disk. Generally there is a dark shading adjacent to the terminator, as in visual observations. Perhaps the most striking features of Ross's photographs are the departures of the crescent from a symmetric form. Especially where there are bright features the planetary limb protrudes markedly. On the other hand, when there are nearby dark features, the terminator has a serrated appearance. The bright protruding features are prominent near the apparent poles, especially near the southern pole; they were also detected visually by such early observers as Schröter and Trouvelot, who explained them as enormous mountains, 60 or more kilometers high. Ross proposed the more likely explanation that the protrusions are areas of atmospheric haze surrounding the planetary poles, such as exist around Mars. This, in turn, implies an appreciable difference in temperature between the equator and the poles. It would be of interest to determine the composition of the polar haze. More recent ultraviolet photographs show three bright and three dark bands, roughly perpendicular to the terminator and extending across the entire visible hemisphere of Venus. The inclination of these bands to the plane of the Cytherean orbit is estimated by Kuiper (*22*) at 32° and by Richardson (*23*) at 14°. The difference between these values emphasizes the observational difficulties.

Ross attributed the presence of band structure to atmospheric circulation, as has been suggested for the Jovian planets. If the explanation is correct, then the speed of rotation at the equator must exceed the speed of random atmospheric winds (*17*), giving a maximum period of rotation of a few weeks. The minimum period is obtained from the absence of a rotational Doppler shift at the planetary limb (*24*). The true period of rotation is probably between 5 and 30 days.

A potentially effective method of studying the Cytherean cloud layer is the determination of the polarization of sunlight reflected from Venus to Earth, as a function of the phase of Venus. The polarization curve of Venus in integrated light was first obtained by Lyot (*25*), who attempted to reproduce it in the laboratory, employing a wide range of substances. He found that for fine mists of water, decreasing the size of the droplets caused the polarization curve increasingly to resemble that of Venus. However, before even a rough fit was obtained, the droplets became unstable. Lyot therefore prepared colloidal suspensions of bromonaphthalene, which have the same differential index of refraction as water in air, and found that droplets of 2-micron radius were required to give an approximate fit with the Venus observations. However, this fit was rather poor; for certain

phase angles, even the signs of the laboratory and Cytherean polarizations disagreed. Furthermore, the near-infrared polarization curve of Venus differs markedly from the theoretically predicted curve for liquid water droplets (*26*). The polarimetric evidence argues against a liquid water cloud layer on Venus, but at the same time no substance is known which provides a better fit to the polarization curve of Venus than do droplets of water. More work is needed, both on the polarimetry of laboratory suspensions of liquids and crystals at low temperatures and in providing the Venus polarization as a function of wavelength (*27*).

The polarization varies over the disk of Venus. Regions of high (negative) polarization are localized in the apparent polar regions, and correlation of high polarization with the bright visual and photographic features in the same regions is tempting. However, there appears to be no consistent difference in polarization between the poles (*28*), while the bright areas near the apparent south pole are usually more brilliant than those near the apparent north pole. Indeed, comparison of visual and polarimetric observations made on the same day shows no clear correlation, and it is possible that the particles responsible for the polarization are different from those responsible for the visual and photographic cloud layer.

The problem of the composition of the clouds has been approached in other ways. The visual albedo of Venus, corrected for yellow coloration, has been estimated to be 0.68 ± 0.04 (*26*). The only common condensable or sublimable substance which is known to have such high reflectivities at the temperatures of the Cytherean cloud layer is water, either as droplets or as ice crystals. Carbon dioxide sublimation is unlikely in the Cytherean atmosphere, because at the carbon dioxide pressures of the cloud layer, temperatures of about 165°K are required for saturation; even in the polar regions there is no evidence for cloud temperatures so low as this. A condensable or sublimable cloud layer is suggested by the polar haze. Noncondensable substances which have sufficiently high reflectivities include quartz, Al_2O_3, CaO, $MgCO_3$ and a number of other geochemically less abundant materials, but it is improbable that there is large-scale preferential production of such substances on Venus (*29*). A variety of molecules has been suggested to explain the yellow coloration, among them polymerized carbon suboxide (*26*), ammonium nitrite (*30*), and nitrogen dioxide. Because these substances have low reflectivities, it is unlikely that the clouds are composed primarily of any of them.

The presence of large quantities of polymerized carbon suboxide is further

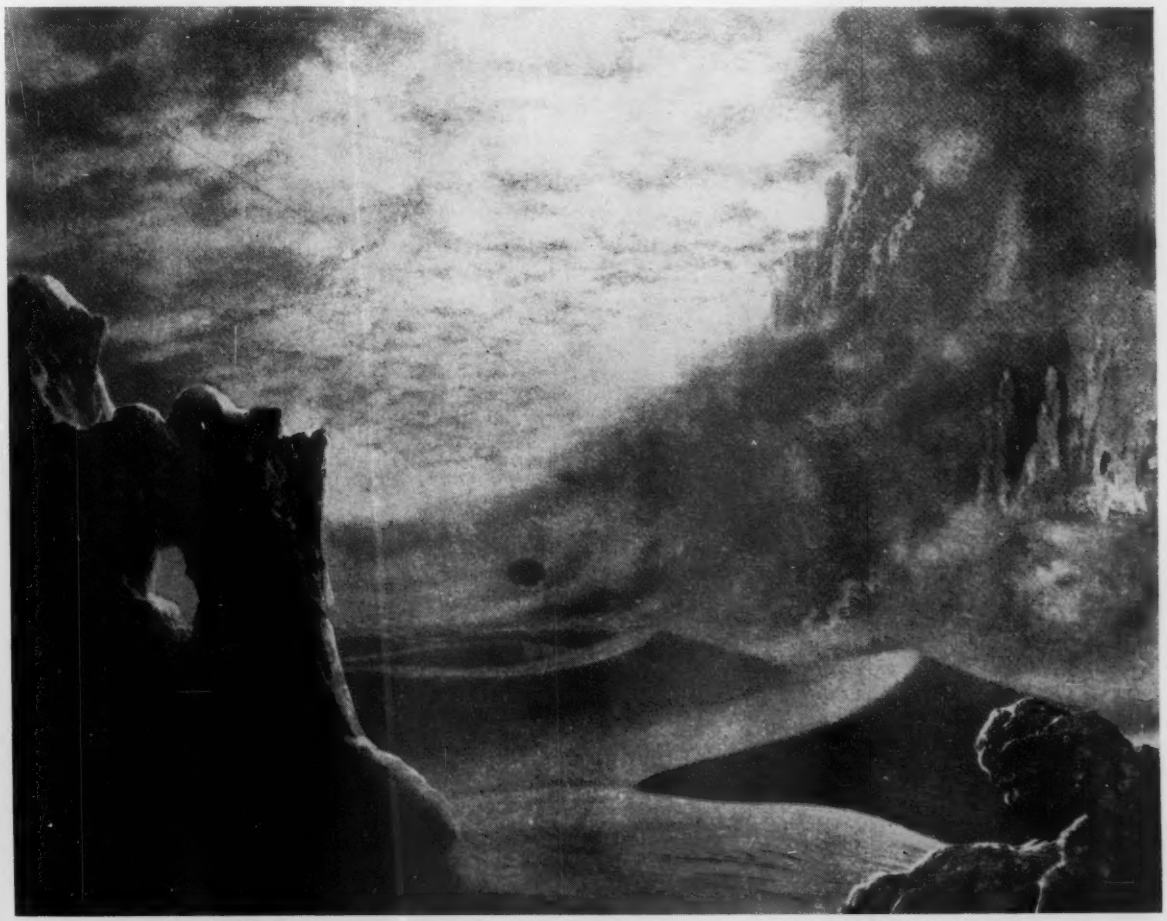


Fig. 1. A painting by Chesley Bonestell showing possible surface conditions on Venus according to the dust-bowl hypothesis. [From a painting by C. Bonestell, in *The Conquest of Space* (Viking Press, New York, 1950)]

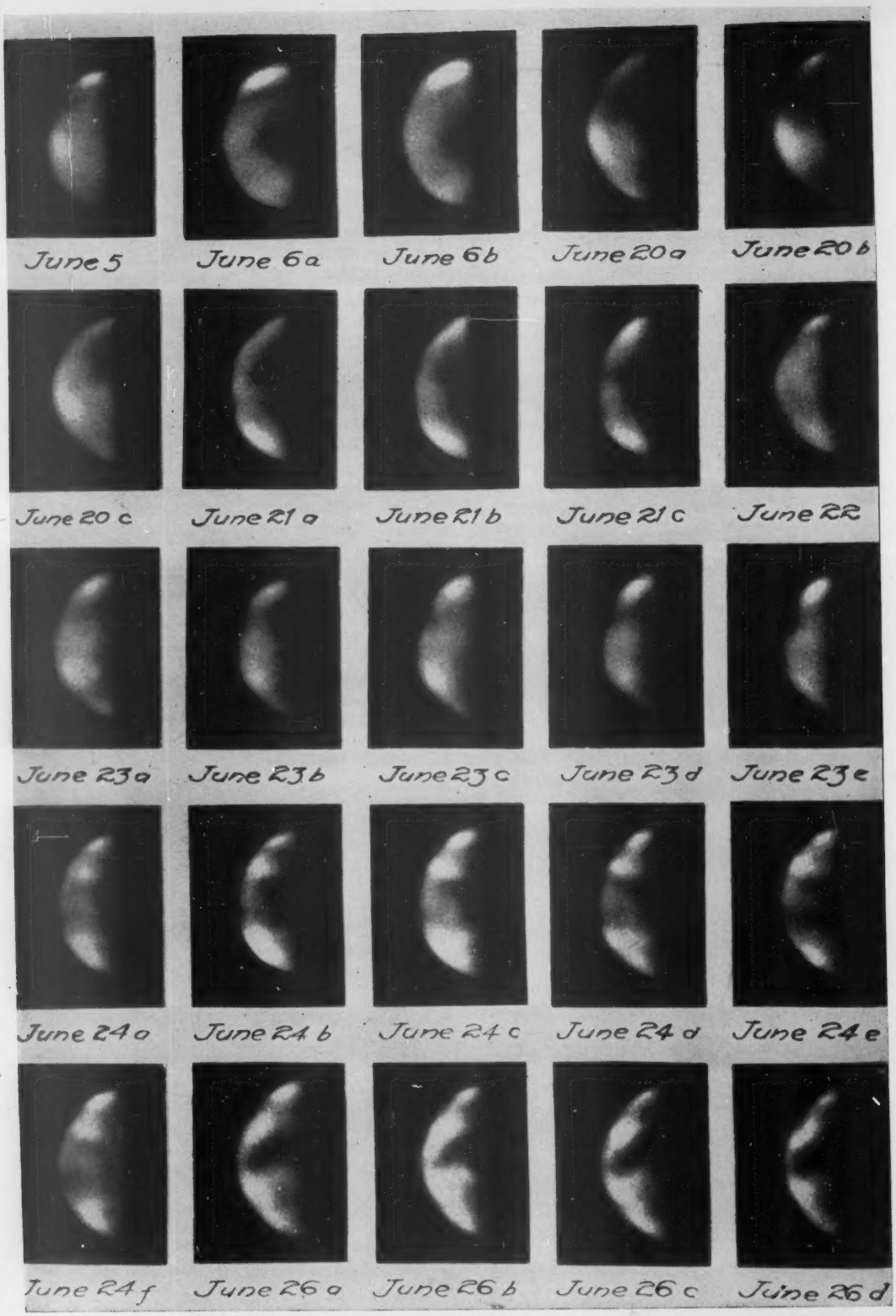
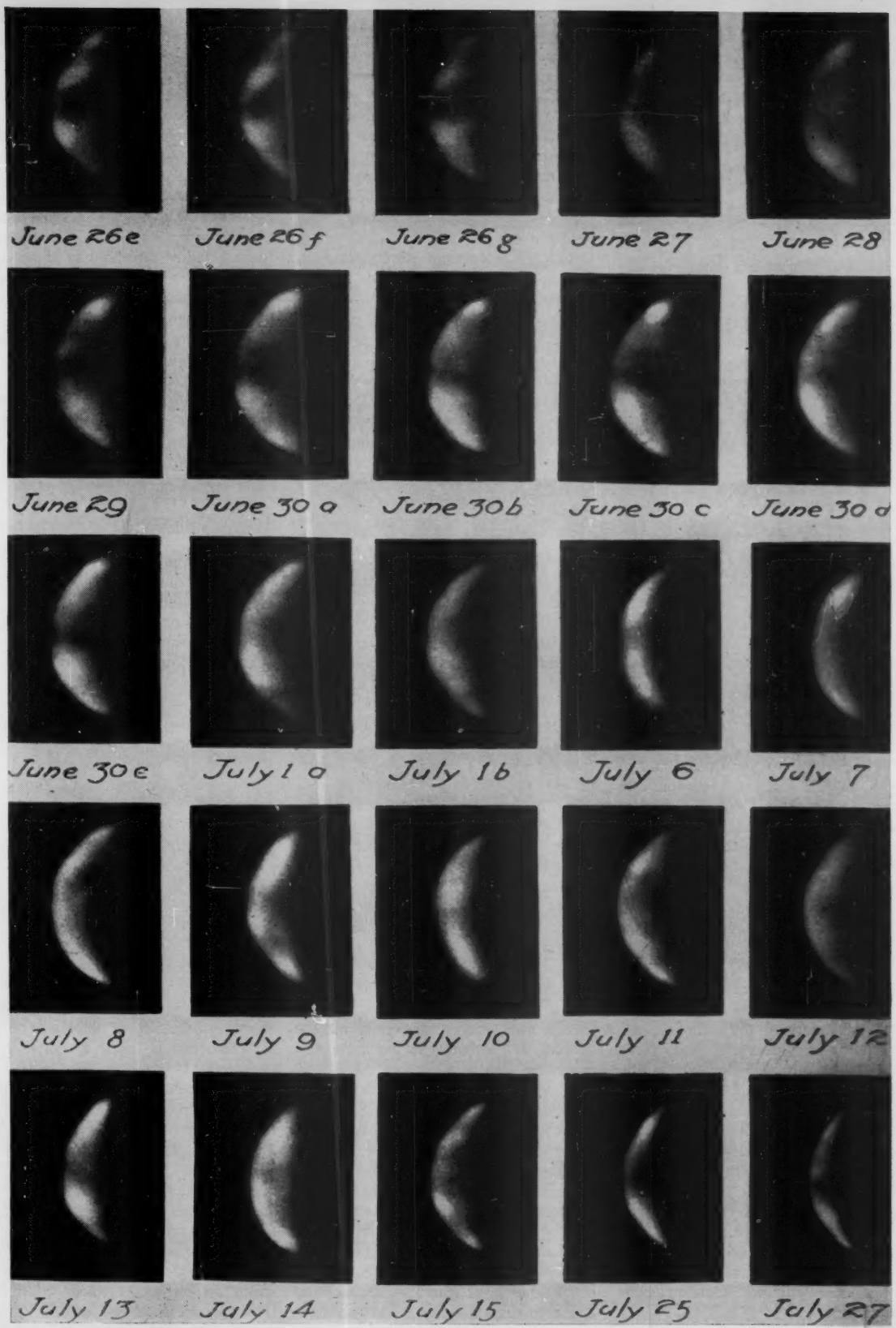


Fig. 2. Sequence of ultraviolet photographs of Venus taken with the 60- and 100-inch reflectors at Mount Wilson Observatory by F. E. Ross, in June and July 1927. [From *Astrophys. J.* **68**, 57 (1928), with permission from the University of Chicago Press. Copyright 1928 by the University of Chicago]


June 26e
June 26f
June 26g
June 27
June 28
June 29
June 30a
June 30b
June 30c
June 30d
June 30e
July 1a
July 1b
July 6
July 7
July 8
July 9
July 10
July 11
July 12
July 13
July 14
July 15
July 25
July 27

improbable because the monomer has a set of fairly strong absorption features extending to wavelengths just short of 3350 angstroms, while all ultraviolet spectra of Venus for this region have been negative; and also because in the photoproduction of each monomer by

$$2\ CO + CO_2 + h\nu \rightarrow C_3O_2 + O_2$$

a molecule of oxygen is formed, while searches for molecular oxygen have also been unsuccessful. On the other hand, small amounts of polymerized carbon suboxide $(C_3O_2)_n$ must certainly be produced and may contribute to the coloration of the clouds. The surprisingly low intensity of the carbon dioxide absorption bands in the 8- to 13-micron region (*31*) suggests the presence of a substance which is transparent to the photographic infrared and opaque to the thermocouple infrared; although it is possible that $(C_3O_2)_n$ can serve this function, the lack of unambiguous laboratory spectra of the polymer (*31*) precludes a more definitive identification. Since polymerized carbon suboxide absorbs in the near-ultraviolet, it is also a likely candidate for the hypothesized high-altitude dark clouds (*32*). Heyden, Kiess, and Kiess (*33*) have called attention to a broad continuum in the near-ultraviolet spectrum of Venus, which they attribute to N_2O_4. Since nitrogen tetroxide is readily dissociated, at wavelengths of less than 3000 angstroms, to the dioxide and to atomic oxygen, it might be thought that a plentiful source of NO_2 is available. However, the identification of the tetroxide must be regarded as extremely tentative, since Kiess and Corliss (*34*) have announced a very similar feature in the spectrum of Jupiter, where so highly oxidized a molecule as N_2O_4 certainly could not escape reduction at all atmospheric levels.

If the surface temperature of Venus is near 600°K (see the discussion below), the theory of the radiation balance implies an ice-crystal cloud layer some 30 or 40 kilometers above the surface (*35*). The predicted cloud temperature is equal to the thermocouple temperature, and the theoretical saturation abundance of water vapor above the clouds agrees with the balloon abundances of water. The reflectivity of such a cloud layer is high enough to explain the Cytherean albedo, but insufficient data are available on the polarization of ice crystals to make comparisons with the Venus polarization curve.

Optical Temperatures

The radiation emitted from Venus at various wavelengths has been analyzed, and corresponding temperatures have been obtained. However, the radiation at a given wavelength may be the integration of emission from many altitudes in the Cytherean atmosphere, and for this reason considerable caution must be exercised in relating these temperatures to specific atmospheric levels. Thermocouple observations in the 8- to 13-micron window in the terrestrial atmosphere yield mean temperatures for the disk of Venus of about 234 ± 10°K (*31*, *36*). The fact that the 10-micron carbon dioxide bands are probably not opaque above the cloud layer (*32*) implies that the cloud temperature is close to the thermocouple temperature. The unilluminated hemisphere has almost the same temperature as the illuminated hemisphere; this circumstance has suggested (*18*) that the period of rotation must be much less than the period of revolution—otherwise the bright side would grow much warmer than the dark side. However, this conclusion is not necessarily valid if violent interhemispheric circulation exists, as indeed the banded structure in ultraviolet photographs would seem to indicate. It is also possible that the temperature at a given atmospheric depth is much lower on the dark than on the bright side but that the observer sees to greater depths in the unilluminated hemisphere because of a sinking of the cloud layer, and so obtains misleadingly high composite temperatures. Evidence exists that the bright ultraviolet clouds in the apparent polar regions are colder than clouds in neighboring areas (*31*), as would be expected if the clouds originate from the condensation or sublimation of a gas.

Thermocouple tracings of the disk of Venus show distinct limb darkening, the intensity falling off approximately as $(\cos\theta)^{\frac{1}{4}}$, where θ is the angle between the line of sight and the planetary vertical (*31*). From this law of limb darkening, the vertical temperature distribution immediately above the cloud layer can be computed; the atmosphere turns out to be nearly isothermal above the cloud layer, the temperature decreasing only as the one-eighth power of the optical depth (*37*). The true lapse rate is only about half the adiabatic lapse rate. In the absence of an ozone layer (there should be little ozone in an atmosphere with no free oxygen) the temperature will remain roughly constant above the cloud layer until dissociation and ionization by short-wavelength solar radiation become significant.

Carbon dioxide will begin to dissociate above unit optical depth with light of wavelengths of less than 1692 angstroms, and the atomic oxygen produced will be ionized in the Schumann-Runge continuum. The absorption cross-section of carbon dioxide just short of the photo-dissociation threshold is about 10^{-20} square centimeters; therefore, the temperature commences to increase, and the Cytherean ionosphere begins at the level above which there is about 1 cm-atm of carbon dioxide. From observations of the occultation of Regulus by Venus (*38*) it is known that there is about 1.7 cm-atm of atmosphere at an altitude of some 70 kilometers above the cloud layer. From the same observations a scale height of 6.8 kilometers has been obtained for this level. Thus, if there is negligible photodissociation at this altitude, the ambient temperature is about 300°K; if carbon dioxide is completely photodissociated, the temperature is about 150°K. The limb-darkening evidence that the atmosphere is approximately isothermal above the cloud layer suggests that the temperature 70 kilometers above the cloud layer probably lies a few tens of degrees below 230°K. An intermediate level of photodissociation is therefore indicated, as would be expected for such a transition region. (It should be mentioned, however, that the $T \propto p^{\frac{1}{4}}$ law inferred from limb darkening cannot apply high above the cloud layer and still be consistent with the occultation data.) If the cloud layer is a few tens of kilometers above the surface, then the Cytherean ionosphere begins at altitudes comparable to those of the terrestrial ionosphere.

Molecular vibration bands show rotational fine structure, which, if a Boltzmann distribution of energy levels is assumed, can be used to derive temperatures. If equipartition exists between rotational and translational energies, the rotational temperature so derived will be the appropriate gas kinetic temperature. On the basis of an assumed radiative transfer in an optically thick Cytherean atmosphere which scatters radiation isotropically, a composite rotational temperature of 285 ± 9°K has been derived from the carbon dioxide bands in the 8000-angstrom region (*39*). From considerations of pressure broadening of the

spectral lines in an adiabatic atmosphere, the temperature at the bottom of the layer emitting the 8000-angstrom features is estimated at 320°K (*26*). The rotational temperatures must arise from deeper levels than the thermocouple temperatures, probably from beneath the visible cloud layer. This is then evidence for the transparency of the visible cloud layer in the near infrared; accordingly, observations at near-infrared wavelengths which lack molecular absorption features should provide information on the lower Cytherean atmosphere.

Microwave Temperatures

Since the 1956 inferior conjunction, when Mayer, McCullough, and Sloanaker (*40*) made their first observations at 3.15 centimeters, measurements have been made of the absolute intensity of the microwave radio emission from Venus. If one assumes *ad hoc* that the signal is due to black-body radiation, a brightness temperature can be derived from the Planck distribution. If the assumption of black-body emission is correct, the brightness temperature should be independent of frequency. Between 3 and 21 centimeters, the brightness temperature proves to be constant within limits of experimental error, and the Venus radio spectrum is inconsistent with known sources of nonthermal emission, such as cyclotron or synchrotron radiation from charged particles trapped in a Cytherean Van Allen belt (*35*). The mean black-body temperature for these wavelengths is approximately 600 ± 50°K, possibly depending somewhat on the phase of Venus. On the other hand, observations near 8 millimeters appear to give significantly lower brightness temperatures; at 8.6 millimeters, observations with the 10-foot radio telescope of the Naval Research Laboratory give a brightness temperature of 410 ± 160°K (*41*), while observations at 8 millimeters with the 22-meter telescope of the Lebedev Institute of Physics yield a mean brightness temperature of 350 ± 70°K (*42*). The Lebedev data also show a marked phase effect, increasing from about 315°K near conjunction to about 430°K two months later.

There are two possible explanations of the thermal radiation at centimeter wavelengths and two corresponding explanations of the lower temperatures at millimeter wavelengths. In the first case, the thermal emission might arise from free-free transitions in a Cytherean ionosphere at an electron temperature of 600°K. To explain the low temperatures near 8 millimeters we must assume that the ionosphere becomes transparent at a wavelength less than about 1 centimeter, so that in the millimeter region we are observing the Cytherean surface at its temperature of about 350°K. From the required opacity and temperature of the ionosphere it follows that the electron density must be at least 10^9 per cubic centimeter (*43*), higher by a factor of 1000 than the electron density of the terrestrial ionosphere. If the electron density is about ten times greater in the illuminated hemisphere, the phase effect can be accounted for exactly (*43*).

Such high electron densities are very difficult to explain. Preliminary calculations indicate that the ionization rates are too low and the recombination rates too high for such electron densities to be produced by solar ultraviolet radiation in a Cytherean atmosphere composed of carbon dioxide, nitrogen, and water vapor. If the electrons are to be produced by solar corpuscular radiation, the strength of the Cytherean magnetic field must be as low as 10^{-3} gauss for ionization by solar protons to be effective; if the field strength is greater, the solar wind will be magnetically deflected. If the ionization is produced by solar protons, there is still a difficulty in accounting for the low recombination coefficients, but the difficulty is not so severe as in the case of ionization by ultraviolet radiation. In addition, there is evidence that the magnetic activity on the earth declines during inferior conjunctions of Venus (*44*); if this phenomenon is interpreted as the deflection of solar protons by the Cytherean magnetic field, it follows that the field strength on Venus is of the order of a few gauss, and that the Cytherean ionosphere cannot arise from the solar wind. However, these measurements of the Cytherean magnetic field are very marginal. There is some radar evidence for the existence of a dense Cytherean ionosphere (*43*), but it is also marginal. For these reasons it appears somewhat unlikely but not entirely impossible that the 600°K emission arises from the ionosphere of Venus.

The alternative explanation is that the surface of Venus is at 600°K, or perhaps at a somewhat higher temperature if allowance is made for phase effects and for the possibility that the surface emissivity differs from unity. Molecular absorption and particle scattering would decrease the apparent temperatures in the millimeter region. The 8-millimeter phase effect would then be attributable to a condensable or sublimable cloud layer, which, if analogous to terrestrial clouds, is transparent at centimeter wavelengths but has a nonzero opacity in the millimeter region. In the illuminated hemisphere it must be supposed that cloud vaporization increases, and the attenuation of emission from the surface declines.

However, the existence of such high surface temperatures must be explained before this model is acceptable. The radiation temperature of an airless planet with the albedo and distance from the sun of Venus is about 250°K, if the period of rotation is a few weeks. The high surface temperature must be due to a very efficient greenhouse effect: Visible radiation strikes the surface and increases its temperature, but the infrared radiation emitted by the surface does not readily escape to space, because of atmospheric absorption. If the atmosphere is assumed to be in convective equilibrium below the effective reflecting layer in the 8000 angstrom bands, there are 18 km-atm of carbon dioxide above the surface; however, this is still insufficient by many orders of magnitude for producing the required greenhouse effect (*35*). Absorption is required in the region between 20 and 40 microns, and the only likely molecule which absorbs in this wavelength interval is water. The requisite total abundance of water vapor in the Cytherean atmosphere is between 1 and 10 grams per square centimeter; saturation and ice-crystal cloud formation occur at the thermocouple temperature of the Cytherean cloud layer and give approximately the balloon water-vapor abundance above the clouds (*35*). Despite an absolute water-vapor abundance of the same order as the earth's, the surface temperature is so high that the relative humidity would be about 10^{-4} percent. On the other hand, if the surface temperature were 350°K, a total abundance of about 0.1 gram per square centimeter would be required for the greenhouse effect; saturation and ice-crystal cloud formation would occur at about 195°K, and it would follow that the clouds are not composed of water, and that the balloon spectroscopy results (*9*) are incorrect. Thus if the visible cloud layer is condensible or sublimable, the ionosphere model of the origin

of the microwave emission becomes untenable. Only with surface temperatures of about 600°K or greater can the requisite greenhouse effect be explained consistently. The Venus overcast is high, not because the cloud bank is very thick, but because breaks in the clouds are very rare. There is no possibility of precipitation reaching the surface; precipitation is always vaporized in the hot lower atmosphere, and ice crystallization occurs again at the cloud layer.

From the equations of radiation balance it follows that 1 km-atm of carbon dioxide is sufficient to raise the ambient temperature some 30°K in the absence of other absorbing gases (*35*). Since 1 km-atm is the abundance of carbon dioxide above the effective reflecting level in the 8000-angstrom bands, the temperature at that level should be raised about 30°K above the radiation temperature, or to approximately 280°K. This is in excellent agreement with the rotational temperature for the same bands, 285 ± 9°K (*39*). The argument also provides strong evidence that the 8000-angstrom bands originate from below the visible cloud layer; otherwise the greenhouse effect would raise the cloud temperature to approximately 280°K.

In Fig. 3, the data given in the foregoing discussion are collected, and the two alternative atmospheric models are presented. The altitudes of the cloud layer—12 and 37 km, respectively, for surface temperatures of 350° and 600°K—are based on an assumed adiabatic lapse rate of 10°K per kilometer in the Cytherean troposphere. Because of the difficulty in accounting for the ionospheric electron densities, the cloud temperatures, and the water-vapor abundance with a surface temperature of 350°K, the higher surface temperature is to be preferred at the present time. A definitive choice between the two models could be made by comparing accurate phase-brightness temperature curves at 8 millimeters and at 3 centimeters with each other and with the occurrence of solar activity, or by scanning the disk of Venus with a fly-by probe. In the latter experiment, strong emission peaks should be detected at the planetary limbs if the radiation at centimeter wavelengths is ionospheric in origin. An experiment to detect limb-brightening near 1 cm is now scheduled by the National Aeronautics and Space Administration for the Mariner A Venus fly-by probe, which will probably be launched in 1962.

Early History and Present Surface Conditions

If the surface temperature is 600°K and the adiabatic lapse rate prevails in the troposphere, the surface pressure of carbon dioxide is about 3 atmospheres. From arguments on general abundance it appears that the value for the partial pressure of nitrogen should be of the same order as the terrestrial value; the total surface pressure on Venus would then be approximately 4 atmospheres. The partial pressure of carbon dioxide is so much greater than the equilibrium partial pressure (less than 10^{-5} atm) that it is clear that the Urey equilibrium fails on Venus. At these temperatures and pressures there can be no global oceans of water (*7*) or of hydrocarbons (*6*, *16*) to limit the access to silicates of outgassed carbon dioxide. The essential point would seem to be that liquid water is required for the Urey equilibrium, both as a catalyst and as a weathering agent to expose fresh silicates, and that, because of the high temperatures, there is no liquid water on Venus (*45*). This explanation was originally proposed by Urey (*5*) at a time when the surface temperatures were thought to be sufficiently low for liquid water to be present but when all spectroscopic searches for water vapor had been fruitless. It now appears that water is

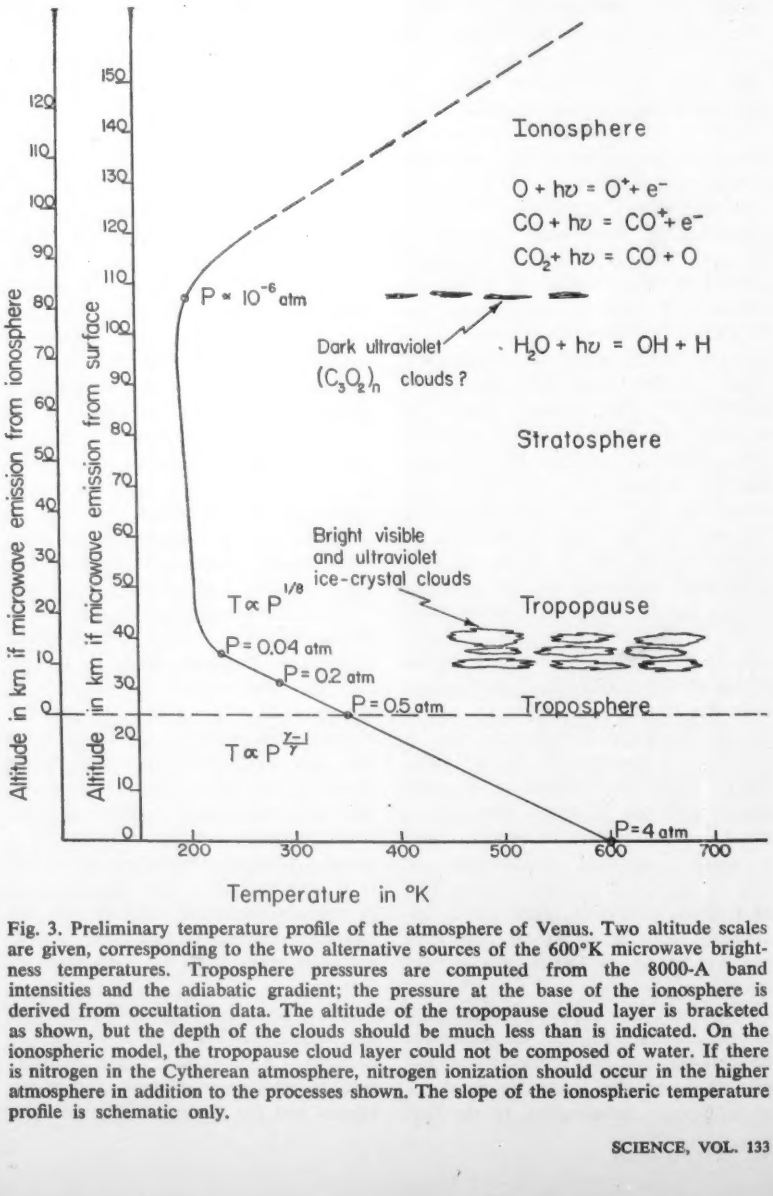


Fig. 3. Preliminary temperature profile of the atmosphere of Venus. Two altitude scales are given, corresponding to the two alternative sources of the 600°K microwave brightness temperatures. Troposphere pressures are computed from the 8000-A band intensities and the adiabatic gradient; the pressure at the base of the ionosphere is derived from occultation data. The altitude of the tropopause cloud layer is bracketed as shown, but the depth of the clouds should be much less than is indicated. On the ionospheric model, the tropopause cloud layer could not be composed of water. If there is nitrogen in the Cytherean atmosphere, nitrogen ionization should occur in the higher atmosphere in addition to the processes shown. The slope of the ionospheric temperature profile is schematic only.

present, but not in the liquid phase. The total abundance of carbon dioxide on Venus is of the same order of magnitude as the total abundance of carbon dioxide in the crust of the earth (*35*); hence, roughly equal amounts of carbon dioxide have been outgassed from the interiors of the two planets during their history, but on the earth the carbon dioxide has been sedimented, while on Venus it has remained in the atmosphere. The ultimate source of the oxygen in Cytherean carbon dioxide must be water (*29*). Through diffusion of water vapor to high altitudes, its photodissociation by solar ultraviolet radiation, and the escape of hydrogen to interplanetary space, enough oxygen has been released during the last 5×10^9 years to account for the present abundance of Cytherean carbon dioxide by the oxidation of reduced carbon compounds (*29*, *35*). The oxygen in terrestrial carbonates, atmospheric oxygen, and the oxygen in water are similarly derived. If much of the primordial Cytherean water had been present at one time on primitive Venus, a very efficient greenhouse effect would have been quickly established and the surface temperature would have risen, vaporizing the water. Thus, it appears that extensive bodies of liquid water and low temperatures could not have occurred together for any appreciable period in the history of Venus.

Consequently, weathering by water must not have occurred, and the present surface erosion on Venus must be due mainly to wind and high temperatures. In the greenhouse model, solar radiation absorbed during the Cytherean day goes mainly into heating the massive atmosphere. As a result the troposphere should be less convective than on Earth, and surface winds should be mild breezes. Such aeolian erosion and the moderate thermal erosion suggested by the microwave phase data suggest that the surface of Venus closely resembles terrestrial desert wastelands. The temperatures are too high for the Carboniferous swamp, the planetary oil field, or the global Seltzer ocean, but, the desert of St. John and Nicholson (*3*) is still roughly consistent with the data (See Fig. 1). From the surface of Venus we might see the sun only dimly; the sky would be completely overcast perhaps 90 percent of the time (*35*), and the high-altitude white clouds would sometimes appear reddened by dust in the lower atmosphere. The Cytherean atmosphere may be transparent in the near ultraviolet. Hot, arid, calm, and overcast, the surface of Venus appears inhospitable for human habitation at the present time.

Life

No known terrestrial microorganisms can survive more than a few minutes' exposure to 600°K; proteins are denatured, deoxyribonucleic acid is depolymerized, and even small organic molecules are dissociated in short periods of time. Temperatures at the poles of Venus are probably not more than 100°K cooler than the mean planetary temperature, and it appears quite certain that terrestrial organisms deposited on the surface of the planet would quickly be killed. Consequently there seems little danger of biological contamination of the surface of Venus (*46*). However, conditions are much more favorable at higher altitudes, especially just beneath the cloud layer, and there is the distinct possibility of biological contamination of the upper Cytherean atmosphere. At such high temperatures, and in the absence of liquid water, it appears very unlikely that there are indigenous surface organisms at the present time. If life based upon carbon-hydrogen-oxygen-nitrogen chemistry ever developed in the early history of Venus, it must subsequently have evolved to subsurface or atmospheric ecological niches. However, since, as has been mentioned, there can have been no appreciable periods of time when Venus had both extensive bodies of water and surface temperatures below the boiling point of water, it is unlikely that life ever arose on Venus.

After the physical enviroment has been thoroughly investigated and if, indeed, Venus proves to be without life, there will exist the prospect of microbiological planetary engineering. To prepare Venus for comfortable human habitation, it is necessary to lower the surface temperature and to increase the partial pressure of molecular oxygen. Both ends could be accomplished if a means were found to dissociate carbon dioxide to oxygen and elemental carbon. The essential difference between the Cytherean and the terrestrial greenhouse effects is the great abundance of carbon dioxide on Venus; the quantities of water vapor in the two atmospheres are approximately equal. Even if ordinary green plants could grow on the surface, the problem would not be solved, because the oxygen evolved in photosynthesis derives from water, and the amount of carbon dioxide in the Cytherean atmosphere is more than 1000 times greater than the amount of water vapor. An organism is needed which can photosynthesize in the high atmosphere of Venus according to the symbolic equation

$$CO_2 + H_2O + \text{light} \rightarrow (CH_2O) + O_2,$$

the oxygen arising from the water. In time, the organisms would be carried to lower atmospheric levels, where, because of the higher temperatures, they would be roasted, decomposing ideally according to the symbolic equation

$$(CH_2O) + \text{heat} \rightarrow C + H_2O$$

Although the oxygen is derived from water, the over-all effect would be to restore the water metabolized in photosynthesis to the atmosphere, and to dissociate carbon dioxide to carbon and oxygen.

Before such a scheme can be seriously considered, much more information must be acquired about the composition and meteorology of the Cytherean atmosphere, and extensive laboratory biological investigations must be performed. Nevertheless, some tentative specifications can be entertained at the present time. In order to have appreciable photosynthesis before thermal dissociation, the life form deposited must be a microorganism. Since there is no liquid water anywhere on Venus, the organism must be able to utilize water vapor (from the atmosphere) or ice crystals (from the cloud layer). The only known microorganisms which photosynthesize evolving molecular oxygen are the algae. It would be desirable to have an organism with resistance to extremes of temperature. Blue-green algae are known to survive immersion in liquid nitrogen, and some forms ordinarily live in hot springs at 80°C. Since there is little likelihood that the microorganisms would find nitrogen in the form of nitrates or ammonia in the Cytherean atmosphere, they would have to be able to fix molecular nitrogen from the atmosphere. The only photosynthetic, nitrogen-fixing, oxygen-evolving, temperature-resistant aerial microorganisms are the blue-green algae, primarily of the Nostocaceae family.

Extensive laboratory experiments should be performed on the ecology of the algae in simulated Cytherean environments. It is necessary to know whether the algae will be able to re-

produce prior to thermal decomposition; whether a complete aerial existence is possible, or if they require the cloud ice-crystals as a substratum; whether a strain can be found which will photosynthesize at low temperatures and high ultraviolet fluxes: whether trace metal requirements can be supplied by meteoritic infall, or whether metals must be provided artificially; and what the products of slow thermal decomposition may be. But it is conceivable that these problems can be solved, and that the microbiological re-engineering of Venus will become possible. Such a step should be taken only after the present Cytherean environment has been thoroughly explored, to prevent the irreparable loss of unique scientific information. It might be advisable to find suitable controls for the algae, because in the absence of predators and competitors the algae might reproduce at a geometric rate and the entire conversion of carbon dioxide would then be accomplished in relatively short periods of time.

Ideally, we can envisage the seeding of the upper Cytherean atmosphere with appropriate strains of Nostocaceae after exhaustive studies have been performed on the existing environment of Venus. As the carbon dioxide content of the atmosphere falls, the greenhouse effect is rendered less efficient and the surface temperature falls. After the atmospheric temperatures decline sufficiently, the decreasing rate of algal decomposition will reduce the water abundance slightly and permit the surface to cool below the boiling point of water. At this time, the original mechanism becomes inoperative, because the algae are no longer thermally decomposed, but now surface photosynthesis becomes possible. At somewhat lower temperatures, rain will reach the surface, and the Urey equilibrium will be initiated, further reducing the atmospheric content of carbon dioxide to terrestrial values. With a few centimeters of precipitable water in the air, surface temperatures somewhere near room temperature, a breathable atmosphere, and terrestrial microflora awaiting the next ecological succession, Venus will have become a much less forbidding environment than it appears to be at present. Hopefully, by that time we will know with more certainty whether to send a paleobotanist, a mineralogist, a petroleum geologist, or a deep-sea diver (*47*).

References and Notes

1. In the literature one sometimes finds the adjective *Venusian*. This is incorrect; we do not say "Sunian," or "Moonian," or "Earthian." The appropriate adjective would be *Venerean* or *Venereal*, but at least the second of these has been pre-empted by other areas of human activity. The Greek goddess corresponding to Venus is Aphrodite, but the appropriate adjective here, *Aphrodisian* or *Aphrodisiac*, must be excluded for similar reasons. Since diseases and love-philters preceded modern astronomy, we must be content with *Cytherean*, an adjective which has been used by astronomers for more than a century. Cythera is the Ionian island onto which Aphrodite is said to have emerged from the sea.
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4. W. S. Adams and T. Dunham, *Publs. Astron. Soc. Pacific* **44**, 243 (1932).
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$$MgSiO_3 + CO_2 \underset{}{\overset{H_2O}{\rightleftharpoons}} MgCO_3 + SiO_2$$

and

$$CaSiO_3 + CO_2 \underset{}{\overset{H_2O}{\rightleftharpoons}} CaCO_3 + SiO_2$$

If, at equilibrium, the abundance of CO_2 increases (for example, through volcanism), the rate of carbonate sedimentation also increases; if the abundance of CO_2 decreases, the reactions reverse, and silicate deposition increases. The partial pressures of CO_2 on the earth are within about an order of magnitude of the equilibrium pressures, at room temperature, resulting from the reaction to dolomite given above.
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8. G. Herzberg, in *Atmospheres of the Earth and Planets*, G. P. Kuiper, Ed. (Univ. of Chicago Press, Chicago, rev. ed. 1952), chap. 13. One centimeter-atmosphere (cm-atm) is the amount of gas corresponding to a path 1 cm long at atmospheric pressure and room temperature. Centimeter-atmospheres can be converted to molecules per square centimeter by multiplication with Loschmidt's number. One kilometer-atmosphere (km-atm) equals 10^5 cm-atm.
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47. This study was supported in part by the California Institute of Technology Jet Propulsion Laboratory, National Aeronautics and Space Administration, under subcontract No. NASw-6. Part of the work was performed at the Yerkes Observatory, University of Chicago. I am indebted to Drs. J. W. Chamberlain, D. Jones, G. P. Kuiper, J. Lederberg, S. Scher, R. Wildt, and to my fellow members of the Planetary Atmospheres Study Group, Space Science Board, National Academy of Sciences, for stimulating discussions on many aspects of this subject.

Scientists and American Science Policy

Who speaks for science? The future offers more dilemmas than unequivocal answers in science policy.

Wallace S. Sayre

The phrase "scientists and American science policy" suggests other comparable formulations: soldiers and American military policy, diplomats and American foreign policy, farmers and American farm policy, businessmen and American business policy, educators and American education policy, labor and American labor policy, and a host of other variations. These parallels serve to remind us sharply of the limitations which a democratic order places upon the role of experts as well as upon special interests in the shaping of public policy. If it can be said, for example, that war is too important to be entrusted to the generals and peace too important to be left to the diplomats, then it may be asked whether science policy is not too important to be delegated wholly to the scientists. In a democratic order all policies of significance must secure a wide range of consent, not merely from the general public but also among the many organized groups and institutions that see their interests importantly involved. Scientists do have a special involvement in science policy, but under the rules of a democratic society they have no monopoly in its development or maintenance, nor have they inherently any greater legitimacy or relevance as participants than all the other claimants who aspire to influence the content of science policy.

Scientists, we may assume, aspire to be influential as a group in the determination of public policy over a wide range, especially those elements of public policy which may be described as "science policy." To exercise such influence the scientists must enter the political arena. Scientists in politics encounter the questions posed by the political process to all those who enter: who are they? who speaks for them? what are their goals? what are their strategies?

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Who Are the Scientists?

If scientists are to be influential participants in constructing an American science policy, they will need to be self-conscious participants—that is, they must have a visible and concrete identity. That identity is now vague and elusive—to many scientists as well as to the other groups involved in the policy process. "The scientific community," a phrase often submitted as an identification, is a world of uncertain boundaries.

Who are the members of the scientific community? Is it an open community, hospitable to all who desire to enter, or is it open only to those who meet severe tests of eligibility? More specifically, are there "hard scientists," whose membership is taken for granted, and "soft scientists," whose credentials are dubious? Are physicists and chemists members of the scientific community by right, while other natural scientists must submit additional claims for admission? Do all engineers qualify, or only certain types of engineers? Do doctors of medicine have entry, or only research scientists in medicine? Are social scientists full members of the scientific community? The answer of the moment appears to be that the natural scientists are the most fully accredited members of the science community but that the life scientists and the social scientists regard this as a transient condition of affairs.

The difficulties raised by these questions suggest that "the scientific community" is most often used as a strategic phrase, intended by the user to imply a large number of experts where only a few may in fact exist, or to imply unity of view where disagreement may in fact prevail. The phrase may thus belong in that class of invocations, so familiar to the political process, which summon up numbers and legitimacy for a point of view by asserting that "the American people," or "the public," or "all informed observers," or "the experts" demand this or reject that. There is nothing especially astonishing about this, since all participants in the political process indulge in the stratagem, and each participant learns to discount the claims of others, but there may be ground for mild surprise that the code of science permits its extensive use by scientists either as deliberate strategy or in genuine innocence.

If scientists are themselves uncertain as to who all their fellow scientists are, then some ambiguities attend their relationship to American science policy. Are they a small elite group (for example, the approximately 96,000 named in *American Men of Science* for the physical and biological sciences), or do they number several million (as they do if the engineers, the social scientists, and the medical profession are included)? If scientists want to be among the shapers of American science policy rather than simply the objects of that policy, then they must expect these and similar questions from the other participants in the making of science policy. The spokesmen of science will be asked: For whom do you speak? The scientists themselves confront a prior question: Who are to be the accredited spokesmen for the scientists?

Who Speaks for the Scientists?

The notion of an American science policy, a policy with which the scientists are to be influentially identified, requires the scientists to have leaders who can act as their representatives in that bargaining with public officials and other groups which accompanies the policy-making process. Not every scientist can participate directly in this

process; there is not room enough, nor time enough, for a town meeting of all the scientists with all the other groups that have equally legitimate claims to be present. Some few scientists must be selected to speak for the many, but the scientists may choose these few in many different ways. They may let the science spokesmen nominate themselves; they may let nonscientists select the leaders of science; they may develop nominating and electoral devices for choosing their leaders through the votes of all scientists in a single scientist constituency; they may choose their leaders in numerous specialized constituencies; or they may combine these methods in various ways, or invent still other methods.

Tradition and recent practice have already provided some important patterns of choice. The history of American science is rich with examples of the articulate, self-directing, individual scientist of high prestige who felt it his obligation to speak often and boldly in behalf of science and the scientists. Few scientists, and fewer nonscientists, have been inclined to question his representative role, although his peers in prestige and self-confidence have often publicly challenged his advice. Another pattern has been provided by the habit which high-ranking government science officials have of speaking, from their position of special eminence and authority, for the interests of science as they perceive them; this would seem to be, for example, the primary function of some government science advisers and advisory committees. If these advisers are the spokesmen of the scientists, it is relevant to ask: What role did which scientists have in choosing them? Still another pattern has been demonstrated by the role of the National Academy of Sciences since 1863. This quasi-governmental body of scientists, its membership small and its new members elected on the basis of scientific eminence by those who are already members, has for many decades acted upon the assumption that it could and should speak for the scientists in the realm of public policy. The scientists who are not members of the academy have not invariably agreed that the academy spoke for them, or that its silence was to be taken as neutrality on their part on contemporary issues of science policy.

These patterns of individuals and small, elite groups, some self-nominated and some the designees of government officials, speaking for the scientists have been accompanied by several efforts to establish more comprehensive scientist constituencies from which spokesmen might be chosen. The American Association for the Advancement of Science is the most durable of these constituencies. Its own membership is large, and its affiliated societies enlarge its base. Its officials, and especially its committees and its journal *Science*, often speak eloquently for the values and the priorities of science and the scientists. One of the most dramatic assertions of its representative role as spokesman for the scientists was its 1958 Parliament of Science, assembled in Washington, to consider the proposal for a government department of science and other issues. Some privacy surrounded the identity of the delegates to this "parliament," the method of their selection as representatives of the scientists, the record of their deliberations, and the extent of their agreement upon the terms of the published report of the "parliament." The sense in which the AAAS and its "parliament" are authentic spokesmen for the scientists as a scientific community thus cannot be easily determined, either by scientists or nonscientists. The Federation of American Scientists provides still another variation—an association of scientists quite explicitly committed to participation in the political process.

But the most prevalent pattern for choosing the spokesmen of the scientists is provided by the specialized associations of scientists. The officers and committees and journals of the American Physical Society, the American Chemical Society, the American Institute of Biological Sciences, the Federation of American Scientists, the Engineers' Council, the Association of American Geologists—and perhaps a thousand other specialized societies—provide the scientists with hundreds of spokesmen in their specialized areas of interest. These spokesmen do not often speak with one voice upon a given aspect of science policy, nor do they often if ever concern themselves with the elements of a comprehensive science policy. Instead, the leaders of each specialized society tend to express their views upon that segment of science policy which touches significantly the interests of the society's own members. As spokesmen for the scientists, their voices are often competitive, emphasizing separate priorities, asserting specialized rather than general goals. In this characteristic the associations of the scientists share the pluralistic, fragmented, and internally competitive attributes of the other group participants in the American political process —whether political parties, business, labor, agriculture, the professions, nationality groups, or the governmental bureaucracies.

The leaders of still other groups often speak confidently in policy discussions as surrogates for scientists. The Association of Land Grant Colleges and Universities, the American Association of University Presidents, science laboratories and institutes, and the science communication media are prominent among these groups. Do they, too, hold a watching brief for scientists by the scientists' own choice?

Who, then, speaks for the scientists? The answer would seem to lie somewhere in a broad zone of ambiguity. Only the scientists themselves can identify their authentic spokesmen. If they have already done so, it would seem to have been done privately and to have been kept confidential. When and if the scientists undertake an explicit identification of their spokesmen, it is not improbable that they will conclude that no one can speak for all of them, and that in a democratic society we will all, perforce, continue to be confronted by numerous, competing spokesmen for science, each often claiming to speak for more of the scientific community than he in fact represents.

An American Science Policy

Uncertainty thus surrounds the questions: Who are the scientists and who speaks for them? Ambiguity also characterizes the phrase "American science policy." It is not difficult to cite examples of particular science policies; these exist in abundance—from the patents clause of the Constitution to yesterday's progress reports of the National Science Foundation. But the unity and comprehensiveness implied by the phrase "American science policy" are not achieved by merely consolidating and codifying all these separate items of science policy. Something more than this is quite clearly implied and evidently desired by many of those who speak for the scientists. It may be assumed, then, that an American science policy is something aspired to but not yet achieved by the scientists: a unified, comprehensive, coherent, rational statement of goals and methods for science

in the United States, accepted by and binding upon all the participants in the policy process, and including agreement upon the rules by which the policy may be changed.

The main elements of such a policy might include the following.

1) A preamble, asserting the values of science to society and the nation; a statement defining the boundary line between the governmental and the private sectors in science.

2) A statement of the priorities for science in each of these sectors.

3) A ranking of the competing claims of science education, basic research, and applied research, as well as an assignment of priorities among the fields of science—chemistry, engineering, physics, biology, psychology, economics, and perhaps a score of others.

4) A statement of agreement and action upon the structure, location, and assignments of the science agencies in the Executive branch—for example, a unified science department (or, alternatively, decentralized science agencies) and the relation of such a department or such agencies to the President and the Congress.

5) Explicit statements of governmental procedures intended to reflect the values of scientists in such matters as secrecy, personnel loyalty and security, government contracts and grants for research, definitions of "basic" research, and provisions concerning the "chain-of-command" in science activities, including protection for the autonomy of individual scientists.

If such a body of public policy existed, accepted by the scientists and legitimatized by the President and the Congress in a statute, thus attesting the consent of the nation, then an American science policy in the fullest implications of that phrase would have been established.

Is such a unified and comprehensive policy a feasible goal for American scientists? Do they in fact desire it?

If a 1961 Town Meeting of Science were to be assembled, despite the problems of deciding which scientists were eligible to attend, agreement could no doubt be reached on the preamble to an American science policy. Preambles, like political party platforms, are usually triumphs in ambiguity. A viable consensus could probably also be reached on item 5—the "working conditions" for scientists—although ambiguity would overshadow precision here, too. But beyond these two items the available evidence suggests that there are no other major elements of an American science policy upon which one could expect unanimity, or even a clear majority agreement, among the scientists themselves. The document which might emerge from the work of such an assembly of science would most likely be an unstable mixture of vague agreement and sharp minority dissents, a testament to the pluralism of science and the scientists. And if the scientists are not likely to agree upon a unified science policy, the prospects that the nonscientist participants might develop such a policy are even less convincing. There are no apparent powerful incentives for any other great interest group in the American society to develop a unified, comprehensive science policy. And even if agreement were possible among the scientists, there is no persuasive evidence that they could win consent without major concessions to the competing claims of all the other interests that must, in a democratic order, agree to such a significant allocation of social values and resources.

Unity and comprehensiveness are thus not likely to be the hallmarks of American science policy. Talk of a single, comprehensive "American science policy" has an essentially fictitious quality. There will be many science policies, rather than a master science policy. Diversity, inconsistency, compromise, experimentation, pulling and hauling, competition, and continuous revision in science policies are more predictable continuing characteristics than their antonyms. This has been the history of American science policies and this describes their present state. We are accustomed to view this state of affairs as deplorable. But to live with diversity and accommodations of policy, and yet to be impatient of them, may be the process by which a democratic society achieves progress in science as well as in other fields. In any event, the future seems to offer American scientists more dilemmas than unequivocal answers in science policy.

Persisting Dilemmas for Scientists

Some of these dilemmas may be illustrated by a brief exploration of a few of the choices concerning governmental arrangements for science—choices which some scientists have helped to make in the past, or which nonscientists have made for them, and still other choices which must yet be made.

Science advisers. One of these choices involves the aspirations of scientists to give advice to officials at the highest levels of the national government—advice not simply in an area defined as "science policy" but also concerning those elements of foreign policy, defense policy, and domestic policy to which many scientists believe their specialized knowledge is relevant. These aspirations of scientists were reflected in the establishment of the National Academy of Sciences a hundred years ago and in the creation of the National Research Council almost fifty years ago. They are reflected today in the existence of the office of Special Assistant to the President for Science and Technology, the President's Science Advisory Committee, the office of Science Adviser to the Secretary of State, and the General Advisory Committee of the Atomic Energy Commission. The Council of Economic Advisers is still another example.

Attempts to define the role of these advisory institutions raise several important questions. Is their primary responsibility to advise the officials as an autonomous voice of the scientists, or are they, as agents or colleagues of the officials, to explain official policies to the scientists, or are they to participate in working out those accommodations in policy which will build a bridge of collaboration between scientists and officialdom? The history of these institutions of advice reveals the tensions, as well as the temporary adjustments, between these inherently competitive conceptions of the advisory role. For the scientists the dilemma remains unresolved: an autonomous science adviser is soon at the periphery rather than at the center of policy making; an involved adviser is soon the advocate of all official policy rather than its critic, an ambassador from the officials to the scientists rather than the scientists' spokesman, or at best a broker between the scientists and the officials. The scientists who are dismayed by these hard choices may perhaps find some consolation in knowing that all other groups in a democratic order confront similar frustrations.

A department of science. Another choice involves the recommendation for a unified department of science, or for a department of science and technology. This proposal to concentrate most of the talents and other resources of the scientists in a single agency,

and "to give Science a voice at the Cabinet table," is a strategy supported by the precedents of comparable aspirations in agriculture, business, and labor. But the proposal encounters today, as it has since John Wesley Powell advocated it before the Allison Commission in the 1880's, the stubborn pluralism of the scientists themselves, the uncertainties of the scientists about the boundaries of their interests, and the opposition of government scientists more willing to endure their existing, familiar organizational environment than to risk the unknowns of a new and untested arrangement. With the scientists thus embattled among themselves, neither the nonscientist interest groups nor the public officials now seem likely to take a determined initiative on a question the scientists, as such, cannot decide. A department of science, then, waits upon the unlikely event that the scientists will soon be able, and will find it desirable, to decide who they are, who their accredited spokesmen are, and what their common goals are, and, most important, able to conclude that they are sufficiently unified to risk their separate interests to the leadership and fortunes of a single government institution.

An autonomous science agency. An alternative choice—the creation of an autonomous science agency, but with a limited assignment—has been at least temporarily decided upon. The National Science Foundation has completed its first decade; its durability now appears convincingly demonstrated. The independence of the agency from the supervision of officialdom is not as great as was hoped for by those spokesmen for the scientists who piloted the proposal through the hearings, the amendments, the debates, and the votes of the 79th and 80th Congresses, past the shock of a Presidential veto indicting excessive autonomy, to the eventual compromise enacted by the 81st Congress. Some of the form, and more than a little of the substance, of autonomy was lost along the way. Annual budgets and annual appropriations are continuing reminders that autonomy is limited, even in decisions about kinds and amount of basic research, and even after sputniks gave the agency higher priorities and the scientists greater authority.

A close look at the composition of the National Science Board since 1950 also raises the question of whether the agency does not more nearly reflect the autonomous voice of university and other administrators of science, alumni from the ranks of scientists though they be, than it does the voice of scientists in the classrooms and laboratories. The task of representing the scientists on the Board has apparently, with the passage of time and with the entry of Presidential and other preferences, been entrusted more to surrogates for scientists than was the expressed expectation of the sponsors and the officials in the discussions accompanying the passage of the National Science Foundation Act of 1950. Surrogates perhaps provide "virtual" or "existential" representation for the scientists; other groups in American society must often accept a similar settlement.

Specialized science agencies. Most existing government science organizations represent a different kind of choice for scientists. These science agencies are immersed in the political system of a large department or "independent" agency, the degree of autonomy of the science unit in that system varying widely. The life scientists, for example, occupy many special units in Agriculture, in Health, Education, and Welfare, and in Interior; the nuclear scientists are found in the Atomic Energy Commission, and other physicists and chemists, in the Bureau of Standards; meteorologists staff the Weather Bureau; scientists of many varieties inhabit Defense Department units; while the geologists have their sanctuary in the Geological Survey, the space scientists have theirs in the National Aeronautics and Space Administration, and the economists have theirs in the Council of Economic Advisers. The other social sciences are less visibly accommodated, but they do staff numerous units in Agriculture, in Commerce, in Health, Education, and Welfare, and in Labor.

The leaders of all these science units have links, strong or attenuated as the case may be, to the associations and institutions of scientists outside the government, but inside the departmental or agency system they share the powers of decision and compete for priorities with other members of the executive hierarchy, and they report to congressional committees whose concerns are not confined to questions of science or the preferences of scientists. In these many science enterprises the scientists are partners with nonscientists rather than autonomous decision makers. They may employ the *mystique* and the *expertise* of science as strategies to maximize their autonomous role, but they cannot realistically expect to be more than senior partners. Most frequently they will be compelled to accept the status of equal partner with nonscientist officials; not infrequently they will find they are actually junior partners. Their hopes for autonomy are, in practice, curbed not only by nonscientist officials in the executive hierarchy and by congressional committees but also by the activities of the interest-group associations in the science bureau's own special constituency. Thus, the Bureau of Mines must listen attentively to the American Mining Congress and the United Mine Workers; the Bureau of Standards, to many industry associations; the Weather Bureau, to the Air Transport Association and the Farm Bureau Federation; the Public Health Service, to the American Medical Association and the American Cancer Society; NASA, to the aviation industry associations; the Atomic Energy Commission, to the electric power associations and many contractor groups; and agricultural research bureaus, to the Cotton Council and numerous other commodity associations. Rare is the science bureau which is not required by its political environment to bargain continuously with, and accommodate its aims and its priorities to, the interest groups in its constituency.

Advice to Congress. Science agencies in the Executive branch have occupied most of the attention of scientists. If they are to pursue their aspirations for a more distinctive and influential role in science policy, the scientists will find it necessary to formulate a general strategy concerning advice to Congress from scientists. No congressional committee is now organized and staffed to give exclusive and comprehensive attention to science policy and to listen continuously to scientists, although the House Committee on Science and Astronautics takes a broad view of its science role, and its Senate counterpart may follow suit. Most scientists must pursue their congressional interests across almost the whole range of committees and subcommittees in both Senate and House. If unity and comprehensiveness in congressional action on science are desired—unity such as is sometimes proposed for science in the Executive branch—scientists will be required to choose among several apparent alternatives: they can propose

a joint committee on science and technology, with a wide-ranging jurisdiction over all the concerns of scientists; or they can propose a comprehensive committee on science and technology in each House, rather than a joint committee; or they can aim at the creation of a joint committee on science policy with a more limited assignment, or of such a committee on science policy in each House. If changes like these were to be made in congressional science committees (an event to be anticipated only after long and determined effort), the scientists still could not expect to enjoy a monopoly of attention from the new committees. Those other groups who now share power with the scientists' spokesmen in the numerous specialized committees and subcommittees would follow the scientists into the new arenas of influence. The scientists might, however, hope to have, at least for a time, higher status and legitimacy as spokesmen before such new committees, and they might also hope that their competitors in the new setting might compete with each other as well as with the scientists. The question which would soon confront the scientists, however, would be, could they establish and maintain their own unity of goals and priorities before the new committees? The odds in favor of an affirmative answer do not seem to be high.

Politics Inescapable

Scientists influential in the creation, maintenance, and modification of American science policy are scientists in politics. The spokesmen for the scientists need not be party officials nor candidates for, or occupants of, elective public office, but they will have to be active participants in other phases of the political process—as high government science officials, as science advisers to executive officials, as spokesmen for science policies before committees of Congress, as organizers of opinion through the communication media, as officials and leaders of science associations and institutions. The leaders of the scientists cannot escape politics and remain leaders in science; since their leaders cannot escape politics, the scientists as a whole are in politics too—even their silence is interpreted as acquiescence.

Leading American scientists have long entered the political arena with boldness and success. Convincing examples are provided by the zeal and skill with which the Scientific Lazzaroni piloted the National Academy of Sciences through the Congress and secured Lincoln's signature in 1863; by the subtlety and determination with which Powell secured the establishment of the Geological Survey in 1879 through an appropriation bill rider (a technique that is of the essence of politics); by the frequency with which the Cosmos Club has served as the meeting place of an informal caucus of scientists planning the strategy for a scientists' *coup d'état* in the public interest; by the magisterial role of Vannevar Bush in national science policy; by the sophistication of the Federation of the Atomic Scientists in their 1946 attack upon the May-Johnson bill and their shaping of the terms of the McMahon Act.

Scientists in politics share the problems of other participants in the political process. No special dispensation exempts the scientists from the hard choices and continuing difficulties which the political process imposes upon all those who aspire to shape public policy. One course is to seek to maximize the unity of all scientists and to establish legitimacy for the spokesmen of a unified science community. An alternative is to accept diversity and competitive priorities among scientists and to establish the identity of the separate groups of scientists, establishing the legitimacy of their respective spokesmen. Whichever of these two main roads is chosen, the united or the separated scientists will face the necessity of recruiting allies from among organized groups of nonscientists; the scientists cannot exercise a unilateral dominance in the making of science policy. Alliances are created and maintained at a price; the price takes the form of mutually acceptable accommodations in policy or priorities. Scientists in politics meet with varying fortunes in the process of bargaining with allies and opponents: in the Bureau of Agricultural Economics they find an environment too severe for survival; in the National Institutes of Health, an embarrassment of riches; in the Weather Bureau, high-velocity cross-winds of pressure; in the Geological Survey, an atmosphere of quiet and modest benevolence which has existed for a half century, since the belligerent initial decades; in the Bureau of Standards, a favorable equilibrium of forces, but in the Public Health Service, an unsteady equilibrium. Such variations are the common experiences of most participants in the political process. The conditions which determine the range of variation are best understood, anticipated, and managed by those who are politicians—that is, by those who are expert in the political process.

The 1958 Parliament of Science states the scientists' hopes and fears in persuasive terms.

> This scientific revolution will totally dwarf the Industrial Revolution and the other historical instances of great social change. It will be more compelling, and will pose more urgent problems, because of both the pace and the magnitude of the changes which now impend.
>
> What faces man is not, in any restricted sense, a scientific problem. The problem is one of the relation of science to public policy. Scientific issues are vitally and almost universally involved. The special knowledge of the scientist is necessary, to be sure; but that knowledge would be powerless or dangerous if it did not include all areas of science and if it were not effectively pooled with the contributions of humanists, statesmen, and philosophers and brought to the service of all segments of society.
>
> What is to be done? Scientists certainly have no arrogant illusion that they have the answers. But they do want to help. They are, moreover, convinced that the time is overripe for a more understanding collaboration between their special profession and the rest of society.

The scientists are now inescapably committed to politics if they hope to exercise influence in the shaping of public policy, including science policies. The leaders of the scientists, then, are perforce politicians. As politicians in a democratic order, they are effective in the degree to which they understand the political process, accept its rules, and play their part in the process with more candor than piety, accepting gladly the fact that they are in the battle rather than above it. The spokesmen for science have occasionally lectured the nonscientists, sometimes sternly, upon their obligation to understand science. Perhaps the advice may be reversed: the scientist has an obligation to understand, and to play his significant role forthrightly in, the polity (*1*).

Note

1. In the preparation of this article I have drawn upon the bibliography given below and upon data developed through an extensive series of interviews with science officials in Washington during 1959 and 1960 and from a study of pertinent official and unofficial documents—a project supported in part by the Council for Atomic Age Studies, Columbia University.

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Harry H. Goode, System Engineer

Harry Goode was born in New York City 1 July 1909. The vigor and the alertness to intellectual challenge that characterized his whole life provided very early the motivation and drive that made a university education possible, in the face of difficulties that would have discouraged a lesser spirit. His bachelor's degree in history from New York University, granted in 1931, came just at the beginning of the depression years. During some of those years he was employed as statistician for the New York City Department of Health, for which he became statistician-in-charge in 1941. At other times, like many other young men in those difficult times, he turned informal talents to advantage—he found himself a good shoe salesman; he was for a while a part-time editor; and he not infrequently played a dance-band saxophone for both pleasure and profit.

To another the Department of Health statistical work might have seemed routine, but to him nothing was ever routine. He discovered, for example, a large number of cases of advanced illness which could have been corrected had the patient seen a doctor earlier. Because of a shortage of physicians available for the department's work, the patients had been unable to see a doctor sooner. By elementary statistical techniques (he would never use sophisticated mathematics where elementary or heuristic techniques would suffice) he was able to show conclusively that the effectiveness of the department would be greatly increased if the physicians would see more people and spend less time with each. His recommendations to this effect were not well received because of the risk of an occasional wrong diagnosis. A lesson from this experience he never forgot: that systems have people and people have prejudices.

During this period his characteristic breadth of interest and motivation toward tangible value for society in anything he might do directed his attention to engineering. He attended night school at Cooper Union and graduated with a Bachelor of Chemical Engineering degree in 1941. This educational venture, combined with his experience as a statistician, brought the realization that for him the enduring intellectual challenge would be in applied mathematics, which he then began to study at Columbia University, receiving the M.A. degree in 1945. He started a doctoral dissertation in statistics under Abraham Wald (on the distribution of noncentral t), but the evident importance of the scientific and professional work that was by then engaging his attention drew him away from work for the doctorate, which he ultimately bypassed completely, in that his achievements without the degree carried him far beyond the accomplishments of most people who hold it. These later achievements suggest that, at least in Harry Goode's life, the wide range of intellectual interests covered in his three distinct and contrasting experiences of higher education, together with his exposure to human and social problems in the New York City Department of Health, provided a richness of early experience of much greater value in preparing him for a life work in systems study than could have come from any intensive concentration in a narrow specialty.

Between 1943 and 1945 he and another young mathematician, Leonard Gillman, were the principal staff of a special project for Tufts College for the Navy's Special Devices Center. Their work, in an office in New York City, would today be called "operations research," but that term was not yet in wide use. For example, they set up a scoring system for a gunnery trainer and included the concept, sophisticated for that time, that the value of a hit late in the run should be less than that of one early in the run because the gunner might not survive. Goode and Gillman were an enormously effective team, producing in two years over 100 memoranda and a major treatise on pursuit courses and the mathematics of guidance and interception, which appeared as a 250-page book.

After the war Goode joined the staff of the Navy's Special Devices Center, where he rose rapidly through successive responsibilities to be head of the Special Projects Branch. His work during this period was on flight control simulation and training, aircraft instrumentation, antisubmarine warfare, weapon system design, and computer research. He was among the first to see the great importance computers were to have, and he was instrumental in initiating several major projects, including the Typhoon computer (the world's largest analog computer) and the Whirlwind computer at Massachusetts Institute of Technology, the first truly high-speed digital computer. Understanding the power of computers, he began at this time to formulate some of the principles of what he later called "system engineering," although he later broadened the system approach considerably beyond the computer.

At the beginning of 1950 he came to

the Aeronautical Research Center (later Willow Run Laboratories) of the University of Michigan as head of the System Analysis and Simulation Group. In 1951 he became chief project engineer, and in 1952, director of the Center. This Center, which doubled in size to 600 people under his direction, was a research and development organization interested principally in guided missile systems, which were "systems" in the fullest sense of Harry's definition of the word. Among the many projects on which the Center was engaged at this time (some of them are still classified) was the study that led ultimately to development of the Bomarc missile. His major work was the study of air defense systems which led ultimately to the Air Defense Integrated System (ADIS) Project. This project was Harry's own brain child and represented the embodiment of his ideas on system design. It emphasized the desirability of the evolutionary system design process as distinct from a long leap into techniques of the future; the desirability of designing a system for the environment which would exist at the time the system was installed rather than for the environment existing at the time of the design; and the necessity for having men rather than machines at certain key spots in the system when the system input is not wholly predictable. Harry first achieved national prominence in his unsuccessful fight for the adoption of ADIS.

During these years his thinking about systems had given rise to several articles, including one called "Simulation, Its Place in System Design," which was published in 1951. Shortly afterward he circulated among a few friends a memorandum entitled "First Rough Draft of the Approach to the Introduction," in which he proposed a joint book on systems analysis. The memorandum stated, "Let me note here that I think each of us will gain considerably by attempting to write this book whether we succeed in doing it or not. First, I know of no book in systems on the subject. Second, I believe the subject is not well defined in anyone's mind, but, in so far as I have been able to determine, better here [at Willow Run] than anywhere else. Third, what we write ought to be of help to someone else undertaking this kind of work."

The outline presented in the memorandum was very rough by his own later standards, and he quite underestimated the magnitude of the task. In

Harry H. Goode

1954, however, he was asked to be a part-time professor of electrical engineering at the University of Michigan and to teach a course which he called "Large-Scale System Design." This gave him an opportunity to formalize his thinking about systems, and simultaneously with the organizing of this course he undertook, with a coauthor, the writing of the systems book. This occupied two years and was completed in 1956; the book, *System Engineering*, was published by McGraw-Hill in 1957. It has subsequently been translated into French and Japanese. Harry regarded this book as perhaps his most important achievement. It classified and regularized systems and the system design process and helped to make respectable the new discipline of system engineering. It is now widely recognized that the design, analysis, and evaluation of large-scale and complex systems require techniques and a viewpoint different from those needed for components or simple devices. It was these techniques on which Harry was an expert and this viewpoint of which he was a past master.

In 1955 he resigned his administrative duties at the Willow Run Laboratories in order to take up full-time teaching duties as a professor of electrical engineering of the University of Michigan; the next year he became also professor of industrial engineering. He insisted on being allowed to teach elementary courses in electrical circuits as well as his own graduate courses. Thus he started out to learn this new field—electrical engineering—from the bottom up. At first he missed some of the atmosphere of his important administrative position and complained to a friend and colleague that he did not have as much "gain" as formerly. The friend, who understood his analogy to an amplifier, pointed out that he had as much "gain" as before but that now he was "reactance coupled" instead of "resistance coupled." He learned this lesson well and was soon able to maneuver in the university environment to achieve the many ends which he sought—for example, the setting up of a degree program in management sciences. Although he was a vigorous organizer, his undertakings were always completely selfless, never directed toward the building of a personal empire.

In 1958 he served for a year as technical director of the Systems Division of the Bendix Corporation, maintaining a part-time appointment at the University of Michigan so that he could continue to teach his new course. He then returned to full-time duties in the university's department of electrical engineering. His duties there included teaching, his own research, supervision of doctoral candidates, and involvement in many of the university's sponsored research projects.

He also found time for much work as consultant, although he could not fulfill the many demands for his services. Besides serving many commercial and industrial organizations in this capacity he served the United States Government as consultant to the National Bureau of Standards, the Post Office Department, and the Appropriations Committee of the House of Representatives. He was also chairman of the Committee on Advanced Reconnaissance (Committee WS-117L) of the Air Force. He also gave his time generously to the profession, serving as a member of the administrative committee of the professional group on electronic computers of the Institute of Radio Engineers (IRE) and as a member of the computer advisory committee of the Society of Automotive Engineers and assisting in the work of the committee on feedback controls of the American Institute of Electrical Engineers (AIEE).

His most important professional-society service was as chairman, during recent years, of the National Joint Computer Committee, formed by the AIEE, IRE, and ACM (Association for Computing Machinery), and as this com-

mittee's representative on a similar international committee. He had an important part in drawing up the charter under which these three societies joined to form a new American Federation of Information Processing Societies, linked closely with an international federation similarly named. In 1959 he was one of a group of eight Americans who toured Soviet computer establishments at the invitation of the Soviet Academy of Sciences.

His many papers touched upon statistics, simulation and modeling, vehicular traffic control, and system design. But through his work his principal research efforts were concerned with reaching a better understanding of large-scale systems. In 1959 he agreed to collaborate on a book with chapter headings such as "Historical and social developments of systems," "General system theories and classifications," "Types of systems," "Descriptions of systems," "Characteristics of systems," and "Modes of solutions for system design problems." However, he soon realized that the subject of systems was not yet sufficiently advanced to allow preparation of such a book, and he directed his attention to the preparation for the McGraw-Hill Book Company of a system engineering handbook. The work he started on this project will be carried through to completion by his friends and associates.

In 1931 Harry married Elsie Guggenheim. Their first child, Lisa, was born in 1943, and their second, Erica, in 1953. They were an unusually closely knit group; in spite of his many research and professional activities, he always found time to devote to his family.

On 30 October 1960 Goode's very fruitful life came to an abrupt end in a traffic accident. Fittingly, the memorial service held two days later was conducted in an auditorium in which he had often lectured, located on the campus of the university to which he gave so much.

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Science in the News

Problems in Political Tactics: Tax Proposals for Education; Congress and Science Policy

A tax credit proposal to encourage private contributions to education was introduced last week by Congressman Carroll D. Kearns (R.-Pa.). The proposal is similar to ones endorsed in recent years by the AAAS, the Association of American Colleges, the National Planning Association, and the U.S. Chamber of Commerce.

The proposal is also similar to a number of measures introduced in the current Congress by conservatives, who see the approach as a way to increase support for education without the direct involvement of the Federal Government. Congressman Kearns, for example, suggested that his proposal might reduce the need for federal appropriations by "as much as $2 or $3 billion if private gifts of that amount were forthcoming." On this reasoning Kearns believes his proposal would make unnecessary most of the Administration's program for higher education.

The way the Kearns proposal and related proposals would work is that instead of offering taxpayers only a deduction from their taxable income for educational gifts, it would offer them the option of a direct deduction from their tax bill of a set proportion of their gifts, in this case 91 percent. This means that a low-bracket taxpayer can give $100 to his alma mater at a cost to himself of only $9; the proposal would make it possible for the taxpayer with a modest income to give to education at as little out-of-pocket expense to himself as the taxpayer in the over $400,000 a year income class.

Hopefully, this would lead to a great increase in private giving. In the extreme case, a man who contributes $100 a year to education, and who does not itemize his deductions (that is, who takes the standard 10 percent deduction) is now giving the entire $100 out of his own pocket. If the Kearns proposal went through, he would, in theory at least, increase his giving to $1000 in order to maintain his out-of-pocket expense of $100. Of course, he would also be free to continue giving only $100 and pocket the $91 as a tax windfall.

The Administration has taken no position yet on the bill or others similar to it, and probably will take none unless it is forced to: that is, unless the House Ways and Means Committee, which must initiate Congressional action on bills involving taxation, decides to hold hearings on the bills.

In the recent past such proposals have been quietly buried in the calendar of pending legislation, the fate of the great majority of the thousands of bills introduced every session. If the proposal is taken seriously enough by the Ways and Means Committee to schedule hearings, then the Administration will have to take a position, which will almost certainly be to oppose the bill.

Between 1952 and 1954 Congress doubled the allowable limit for tax deduction for philanthropic gifts from 15 percent to 30 percent of taxable income. Contrary to expectations, this produced no increase in giving. Different influences would be at work under the Kearns proposal, and it would surely produce some increase in giving, but whether it would be a substantial increase is uncertain. Tax rates have, after all, gone up enormously in the last 30 years, and therefore the inducement to tax-free giving has gone up enormously. Yet giving, as a percentage of taxable income, has remained constant at around 4 percent. Unless the increase under the Kearns proposal were more than marginal, the total

amount of private gifts for education would not be substantially increased, while the government's tax revenues would be reduced, thus reducing the amount of money available for federal education programs.

Tax Policy

This uncertainty about how much the proposal would actually increase support for education and how much it would simply add another loophole to the tax code virtually insures that the Treasury under the Kennedy Administration, as it did under Eisenhower, will oppose the plan quietly, by encouraging the Ways and Means Committee to bury it, and openly if hearings are actually held.

Almost everyone agrees that a major rewriting and simplification of the tax code is overdue, and therefore hardly anyone familiar with the difficulty of putting across a tax-reform program is willing to support a new set of deductions unless there is overwhelming evidence that the proposals will really do what they are intended to do, and unless there seems to be no other way to accomplish the objective than by further complicating the tax code.

Neither seems clear in this case, while the likelihood that passage of the proposal would bring a wave of requests to offer the same benefits for contributions to research, to culture, and to good causes generally is quite clear. The difficulty is that although a reasonably good case can be made for almost any of the existing or proposed tax breaks, the total of all of the breaks adds up to a headache for taxpayer and tax collector alike.

The sum of this is that the Kennedy Administration, which has considerably fewer qualms about enlarging the role of the federal government in education than the Eisenhower Administration did, is hardly likely to support an approach which the old Administration rejected, despite its admittedly desirable objective.

Political Tactics

Quite aside from this, the Kearns-type proposals raise a problem of political tactics. It is standard procedure for opponents to any program before Congress, when they are threatened with a defeat, to offer their own scaled-down plan to accomplish whatever is being debated. Congressman Kearns, for example, voted against federal aid for education in the last session. If the scaled-down proposal goes through, it can be argued that there is no longer any need for further action, at least for a few years until we can see whether the more modest program will do the job.

This means that if the Administration were to support the Kearns bill, or related proposals, such as that offered by Senator Goldwater, it would be helping to defeat its own program in return for a program with great public appeal, but one which may not help very much in providing more money for education.

The prospect, then, for Kearns-type legislation is close to nil. It will be opposed on grounds of tax policy, and on grounds that it is no adequate substitute for the direct federal assistance in the Administration's program, and although a President cannot get everything he wants from Congress, on an issue like this there is not much chance that Congress will force him to take something he doesn't want.

The issue also illustrates one reason why Congress is so fond of the committee system. If a proposal with as much popular appeal as the tax credit plan ever reached the floor for a vote, every Congressman would be under heavy pressure to vote for it whether he liked it or not. This awkward situation is avoided when the bill is quietly allowed to die in committee.

Congress and the PSAC

A far deeper problem for the Administration than the tax credit substitutes for its education program is involved in dealing with Congress on science policy. The problem has been quietly growing for several years as science has taken an increasingly important position as a distinct policy area, which makes increasingly significant the considerable lack of understanding between the scientific advisers and the committees on Capitol Hill which decide how much money they can have and where it will be spent.

Two things seem to especially annoy the Congressmen. One is that they do not like to be told just what should be done, but why it should be done, and so far, at least, the scientists have not been as successful as either they or the Congress would like in explaining the why's of the policies they recommend. This is not surprising: scientists, after all, do not always understand the why's of politics, and politics, being in the realm of everyday experience, is a good deal easier to explain to a nonpolitician than science is to a nonscientist.

This apparently unavoidable difficulty is compounded by a second factor, which is the position of the President's Science Advisory Committee, which, as part of the President's office, is shielded from Congressional inquiry by executive privilege.

In one sense this is perfectly normal: not many dispute the fact that it is perfectly proper for a president to have personal advisers on his staff, and that presidents understandably would not want these advisers to be liable to be summoned before a Congressional committee to find out what kind of advice they are giving. In any case, this "executive privilege" is well-established in law, and presidents have regularly invoked it when Congress has tried to find out what is going on in the White House. But it merely stops the inquiring Congressman from getting very far; it does not quite satisfy his curiosity.

What causes misgivings is not that the science adviser is not available in his role of personal adviser to the President. Here his role is similar to that of, say, McGeorge Bundy, the Harvard dean, and W. W. Rostow, the M.I.T. economist, who serve as special assistants for national security affairs.

In this role the science adviser is in every sense simply a member of the personal staff of the President, and Congress recognizes that, curious though it may be about what goes on in the White House, it has no authority to question members of President's staff.

A less clear situation comes up in considering the science adviser in his role as chairman of the Science Advisory Committee. There is no legal question: the committee, its staff, and its special panels, which in the course of a year will use the services of dozens of the leading scientists in the country, all clearly exist to serve the President; there is no statutory requirement that they exist; they are paid out of the general funds available to the President for running his office "to be accounted for solely on his certificate." The whole operation could be scrapped at any time at the whim of the President. But since the committee has become in effect the ranking body concerned with over-all national science policy it is

unthinkable that it, or something like it, should not exist.

The committee has no exact parallel: the National Security Council, for example, is an even more important body concerned with over-all national security policy, but its existence is required by law, there is some public accounting of the money spent on it; and its members, except for the President and Vice President, can be questioned by Congress, not, of course, in their roles as members of the council, but as operating heads of departments concerned with national security affairs.

Role of the Committee

The peculiar position of the Science Advisory Committee is exaggerated here to make clearer the distinction between it and other parallel bodies in the government. Yet the difference is real enough so that one of the leading arguments for establishing a cabinet department of science has become the desire to remove at least some of its functions from the President's confidential staff to a place where Congress can keep a closer eye on how national science policy is made.

The difficulty is compounded by the nature of the advice the committee gives, which falls in an area where neither Congress nor the President has much of the independent expertise which both command on other policy matters. There is some feeling in Congress that this is an area where, more than in any other area, the President is forced simply to take advice, rather than receive information and then make up his own mind. Therefore, this reasoning goes, he and the country should be protected by making sure that it is possible to have independent criticism of the advice he is getting. One way, and perhaps the only way, to do this, and a way which especially appeals to Congress, would be to give Congress some authority to inquire into how policy is being made.

These comments by no means give a full or fair picture of all the factors involved; they are intended only to summarize the kind of misgivings that exist about the role of the committee. An excellent argument can be made for keeping the Science Advisory Committee just the way it is.

But whatever the wisest kind of organization, the President clearly has at least a touchy problem in political tactics in dealing with Congressional feelings on these points.—H.M.

News Notes

Venus Studied with Radio Signals

First success in a 2-month experiment in which radio signals are being used to study the planet Venus has been announced by the National Aeronautics and Space Administration. Hugh L. Dryden, deputy administrator of NASA, and William H. Pickering, director of the Jet Propulsion Laboratory, announced reception of strong, clear radio signals reflected back to earth from Venus in a 70-million-mile round trip that took about 6½ minutes. The transmission was completed at 9:34 P.M. EST, 10 March, at the Jet Propulsion Laboratory's Goldstone Tracking Station, 50 miles north of Barstow, Calif., in the Mohave Desert. The Jet Propulsion Laboratory is operated for NASA by California Institute of Technology.

Dryden said that signals have been bounced off Venus in other experiments but that this is the first time such signals have been immediately detectable without elaborate analysis and processing. The objectives of the experiment are to (i) determine whether Venus spins on its axis and, if so, at what speed of rotation; (ii) determine the orientation of the planet's spin axis; (iii) investigate the nature of the surface of Venus as determined by the reflectivity of its surface; and (iv) further define the measuring stick of the universe, the approximately 93-million-mile astronomical unit. (The length of the unit, the mean distance from the earth to the sun, has been defined only to within about 10,000 miles.)

Venus, which lies between Mercury and the earth, is the planet nearest the earth and long has been of interest to astronomers, but its atmosphere of dense clouds has hindered observation. Every 19 months Venus approaches to within about 26.2 million miles of the earth; its maximum distance from the earth is 162 million miles. This closest approach, known as the inferior conjunction, occurs this year on 11 April.

At the present stage of development of radio communication it is impractical to attempt radio contact with Venus except during comparatively brief periods before and after inferior conjunction. However, the Goldstone experiment will continue for several weeks after the 11 April date.

AEC Acts to Separate Regulatory Function

Chairman Glenn T. Seaborg of the Atomic Energy Commission has announced that the commission has acted to separate its regulatory function from the operational and developmental functions at the general manager level. The first step in this separation is the designation of Harold L. Price as acting director of regulation; he will report directly to the commission. Price has been director of the Division of Licensing and Regulation.

Price has been authorized to discharge the licensing and other regulatory functions of the commission, other than those where the final decision rests with the hearing examiner or the commission, or those which involve the commission's authority to approve the issuance of regulations. General manager A. R. Luedecke will continue to administer the commission's operational and developmental activities.

In a report made to the Joint Committee on Atomic Energy of Congress, in February, the commission outlined certain contemplated changes in the regulatory organization. This first step is taken in accord with that plan. These actions do not in any way prejudice possible additional steps the commission might wish to take following consideration of a similar study being completed by the staff of the Joint Committee on Atomic Energy.

Price will submit to the commission a report on the staffing of the regulatory function. He will have the cooperation of the general manager and the general counsel in the preparation of this report. Pending completion of the staffing report, the Divisions of Licensing and Regulation and of Compliance and the Office of Health and Safety are transferred to the acting director of regulation.

Robert Lowenstein has been designated acting director of the Division of Licensing and Regulation, succeeding Price. Lowenstein has been serving as counsel for the division.

AAAS Socio-Psychological Prize

Through the generosity of an anonymous donor, the AAAS offers an annual prize of $1000 for a meritorious essay in sociopsychological inquiry. Previous winners of this prize and the titles of their essays have been: Arnold

M. Rose, "A theory of social organization and disorganization"; Yehudi A. Cohen, "Food and its vicissitudes: a cross-cultural study of sharing and nonsharing in sixty folk societies"; Herbert C. Kelman, "Compliance, identification, and internalization: a theoretical and experimental approach to the study of social influence"; Irving A. Taylor, "Similarities in the structure of extreme social attitudes"; Stanley Schachter, "The psychology of affiliation"; and Robert Rosenthal, "Three experiments in experimenter bias."

The conditions of competition for the prize to be awarded at the 1961 annual meeting, to be held in Denver, Colorado, from 26 to 31 December, are as follows.

1) The contribution should further the comprehension of the psychological-social-cultural behavior of human beings—the relationships of these hyphenated words being an essential part of the inquiry. Whether the contributor considers himself to be an anthropologist, a psychologist, a sociologist, or a member of some other group is unimportant as long as his essay deals with basic observation and construction in the area variously known as social process, group behavior, or interpersonal behavior. For ease of reference in the rest of this statement, this general area will be called "social behavior."

2) The prize is offered to encourage studies and analyses of social behavior based on explicitly stated assumptions or postulates, which lead to experimentally verifiable conclusions or deductions. In other words, it is a prize intended to encourage in social inquiry the development and application of dependable methodology analogous to the methods that have proved so fruitful in the natural sciences. This is not to state that the methods of any of the natural sciences are to be transferred without change to the study of social behavior, but rather that the development of a science of social behavior is fostered through observation guided by explicit postulates, which in turn are firmly grounded on prior observations. It may be taken for granted that such postulates will include a spatial-temporal framework for the inquiry. It may properly be added that the essay should foster liberation from philosophic-academic conventions and from dogmatic boundaries between different disciplines.

3) Hitherto unpublished manuscripts are eligible, as are manuscripts that have been published since 1 January 1960. Entries may be of any length, but each should present a completed analysis of a problem, the relevant data, and an interpretation of the data in terms of the postulates with which the study began. Preference will be given to manuscripts not over 50,000 words in length. Entries may be submitted by the author himself or by another person on his behalf.

4) Entries will be judged by a committee of three persons considered well qualified to judge material in this field. The judges will be selected by a management committee consisting of the chairman and the secretary of Section K and the executive officer of the AAAS. The committee of judges reserves the right to withhold the prize if no worthy essay is submitted.

5) Entries should be sent to Dael Wolfle, Executive Officer, American Association for the Advancement of Science, 1515 Massachusetts Ave., NW, Washington 5, D.C. Entries should be submitted in quadruplicate. Each entry should be accompanied by six copies of an abstract not to exceed 1200 words in length. The name of the author should not appear anywhere on the entry itself but should be enclosed on a separate sheet of paper which also gives the author's address and the title of his essay. Entrants who wish to have their manuscripts returned should include a note to that effect and the necessary postage. To be eligible for consideration for the prize that will be awarded at the 1961 annual meeting of the Association, entries must be received *not later than 1 September 1961.*

Serials Record Center Planned by Biological Sciences Institute

Establishment of a Biological Serials Record Center to facilitate communication between scientists has been given first priority by the Biological Sciences Communication Project of the American Institute of Biological Sciences. Unless the scientific intercommunication problem is solved, the committee reports, scientists of the future may find that they are "buried under printed paper" and that their research is aimed at finding knowledge which already is known.

The Serials Record Center would be charged with the task of collecting and disseminating detailed information regarding all serial or periodical publications in biology. Estimates of the number of these journals range from 25,000 to 50,000. The publications cover all aspects of biological research, including clinical medicine, agricultural science, and various botanical and zoological disciplines.

The site of the proposed center, the estimated cost of operation, and other details will be determined in the near future. The Biological Sciences Communication Project is headed by Charles W. Shilling. Its office is at AIBS headquarters, 2000 P St., NW, Washington 6, D.C. Members of the project's executive committee include J. Roger Porter, Fred R. Cagle, and Foster Mohrhardt. Lincoln Constance of the University of California is chairman of the advisory committee.

News Briefs

Geneva test-ban conference. The conference on the discontinuance of nuclear weapon tests resumed its sessions in Geneva on 21 March, with U.S. representative Arthur H. Dean heading this country's delegation. Deputy U.S. representatives are Charles C. Stelle and David H. Popper, both of the United States Mission to the European Office of the United Nations, Geneva.

* * *

IAEA-Norway research project. Sterling Cole, director general of the International Atomic Energy Agency, and Gunnar Randers, managing director of the Norwegian Institute for Atomic Energy, have announced the selection of Raja Ramanna of India to be chairman of the management committee for the joint research program in reactor physics, in which Norway's zero power reactor, NORA, will be utilized. Selection of the chairman of this committee is a matter of mutual choice.

In addition, each of the parties names two committee members. Cole has selected Pierre Louis Balligand (France) and Carlo Salvetti (Italy) as IAEA representatives. Norway has named Olav Kasa and Henrik Ager-Hansen. The committee will hold its first meeting in the beginning of April at Kjeller, Norway, site of the NORA reactor.

* * *

Graduate fellowships. The National Science Foundation has announced the award of 1537 graduate fellowships for the academic year 1961–62. Recip-

ients were selected from 4875 applicants from all parts of the United States and its territories. The foundation also released the names of 1588 persons accorded honorable mention. Of the awards, 347 were made in the life sciences, 1156 were made in the physical sciences (a number of these were in interdisciplinary fields), and 34 were made in certain areas of the social sciences.

Fellows may attend any appropriate nonprofit American or foreign institution. Graduate fellowships provide basic stipends (for 12 months) of $1800 for the first-year level of graduate study, $2000 for intermediate years, and $2200 for the terminal year. Additional allowances are provided for dependents and tuition, as well as for travel.

* * *

AIBS meetings. The annual joint meetings of biological societies sponsored by the American Institute of Biological Sciences will be held this year at Purdue University, 27 August to 1 September. Advance indications are that approximately 5000 scientists will participate and about 1500 research papers will be given, in 200 sessions. Local chairmen are John S. Karling, Wright distinguished professor of biological sciences, and E. C. Stevenson, head of the department of horticultural sciences.

Grants, Fellowships, and Awards

Biological psychiatry. The Society of Biological Psychiatry is offering an annual award which was made possible by the A. E. Bennett Neuropsychiatric Research Foundation. The $500 award —part of which is to be used for traveling expenses to the society's meeting —will preferably be given to a moderately young investigator, not necessarily a member, for recent, unpublished work. The winning paper will be read as part of the annual meeting program and will appear with the other meeting papers in volume 4 of *Biological Psychiatry*. Papers should be submitted in quadruplicate *before 30 April* to Harold E. Himwich, Chairman, Committee of Award, Galesburg State Research Hospital, Galesburg, Ill.

Fulbright awards. The Committee on International Exchange of Persons of the Conference Board of Associated Research Councils has announced the program for the 1962–63 awards, under the Fulbright Act, for university lecturing and advanced research in the following countries of the Pacific, South and Southeastern Asia, and Latin America: Australia and New Zealand; Burma, Ceylon, India, Korea, Pakistan, Philippines, and Thailand; Argentina, Brazil, Chile, Colombia, Ecuador, Paraguay, Peru, and Uruguay. Application forms and detailed program information may be obtained by writing to the committee at the National Academy of Sciences–National Research Council, 2101 Constitution Ave., NW, Washington 25, D.C. Applications should be submitted *before 25 April.*

Heart disease reporting. Entries are now being received for the Howard W. Blakeslee Awards, to be presented by the American Heart Association for outstanding reporting in the field of heart and blood-vessel diseases. The contest year, which began March 1960, came to a close on 28 February. Newspaper and magazine articles, books, radio and television programs, and films published or produced during that period are eligible. Deadline for submission of entries is *1 May.*

The winning entries and the number of awards will be determined by the Blakeslee Awards Committee of the American Heart Association. The awards carry an honorarium of $500 each.

Entries submitted by local daily or weekly newspapers and local radio and television stations will be considered apart from those submitted by national wire services, syndicates, or radio-television networks and will be eligible for awards in separate categories. Entry blanks and rules folders may be obtained from local Heart Associations or from the American Heart Association, 44 E. 23rd St., New York 10, N.Y.

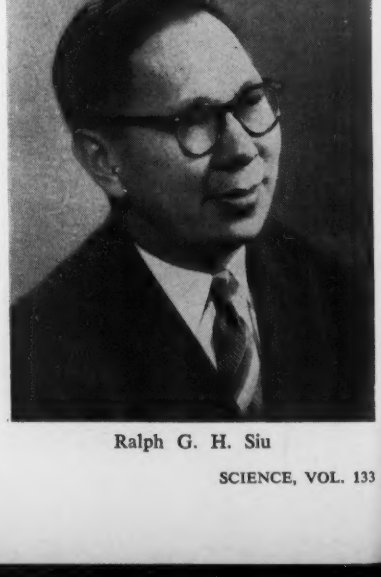
Medical student summer research. The Lederle Laboratories Division of the American Cyanamid Company has announced that it is making available to medical schools throughout the United States and Canada Lederle medical student research fellowships for the year 1961. These fellowships, in amounts not exceeding $600 per year for any one individual, are intended to relieve in part the financial burden of students who desire to devote their summer vacations to research in the preclinical departments.

Applicants must be of good scholastic standing and must have the consent of the faculty member under whose supervision their research is to be conducted. Award recipients will be selected by the dean of the medical school, or by the regularly constituted committee of the faculty charged with such selections. By special permission of the dean or the school's fellowship committee, a student may carry on his research in another medical school, provided that arrangements are previously made with that school.

Scientists in the News

Luis W. Alvarez of the University of California, Berkeley, will receive this year's $5000 Albert Einstein Gold Medal and Award. Alvarez is professor of physics in the department of physics and a leader in the high-energy physics program at the university's Lawrence Radiation Laboratory. The award was announced on 14 March, the anniversary of Einstein's birth, by the trustees of the Lewis and Rosa Strauss Memorial Fund.

Ralph G. H. Siu, technical director of research and development in the Research and Engineering Division, Office of the Quartermaster General, Department of the Army, received one of the highly prized Career Service Awards of the National Civil Service League on 21 March, at a dinner in Washington, D.C. Siu was honored for his work in the radiation preservation of food. The league, a nonpartisan citizens' organization representing the public interest in improving government, this year gave its seventh series of Career Service Awards to ten federal employees chosen for competence, character, and outstanding achievements.



Ralph G. H. Siu

C. R. Barber and **J. A. Hall**, both of the National Physical Laboratory, Teddington, Middlesex, England, are in this country to attend the third Symposium on Temperature, in Columbus, Ohio, 27–31 March, and to spend some time at the National Bureau of Standards, Washington. Barber will also visit Boulder (Colo.), Ottawa, and New York.

J. F. Duke, senior scientific officer, Metallurgy Division, National Physical Laboratory, will attend a conference on the Ultrapurification of Semiconducting Materials, to be held in Boston, 11–13 April. His itinerary includes: Schenectady, N.Y.; Columbus, Ohio; Oak Ridge, Tenn.; Washington, D.C. (20–21 April); Philadelphia; and New York.

J. R. Lake, principal scientific officer, Road Research Laboratory, Harmondsworth, Middlesex, will visit Canada and the United States from 16 April to 17 May. His itinerary includes: Toronto; Hamilton, Ont.; Vancouver, B.C.; Edmonton, Alberta; Ottawa, Ont.; Washington, D.C. (12–13 May); and Chicago, Ill.

J. M. Shewan and **R. Spencer**, senior principal scientific officer and senior scientific officer, respectively, of the Torry Research Station, Aberdeen, Scotland, will attend the International Symposium of Marine Microbiology in Chicago, 20–24 April. Shewan will also spend 2 weeks in Gainesville, Fla.

The John and Mary R. Markle Foundation, New York, has announced the names of the 25 Markle Scholars in Medical Science appointed for 1961–66. For the first time since 1948, when these annual appointments were first made, a woman physician is among those selected. She is **Mary Ellen Avery**, assistant professor of pediatrics at Johns Hopkins University School of Medicine.

The purpose of the program is to help relieve the faculty shortage in medical schools by giving young teachers and investigators academic security and financial assistance early in their careers. Funds appropriated for the program total $750,000. Grants are made to the 25 schools where the scholars will work (beginning 1 July). Each school will receive $30,000, at the rate of $6000 a year for the next 5 years, toward support of the scholar and his research. This year's Markle Scholars, in addition to Mary Ellen Avery, are as follows:

Walter F. Ballinger II, assistant professor (as of 1 July), Jefferson Medical College of Philadelphia, surgery.

André Barbeau, fellow and lecturer, University of Montreal Faculty of Medicine, neurology.

John R. Blinks, associate, Harvard Medical School, pharmacology.

Leon Cander, assistant professor, Hahnemann Medical College, internal medicine.

James D. Case, assistant professor, University of Washington School of Medicine, internal medicine and dermatology.

G. Daniel Copeland, instructor, University of Tennessee College of Medicine, internal medicine.

Frank W. Fitch, assistant professor, University of Chicago, Division of the Biological Sciences, pathology.

J. Alex Haller, Jr., assistant professor (as of 1 July), University of Louisville School of Medicine, surgery.

Monte Gene Holland, assistant professor, Tulane University School of Medicine, ophthalmology.

Ellis B. Keener, instructor, Emory University School of Medicine, neurosurgery.

Henry Neil Kirkman, Jr., assistant professor, University of Oklahoma School of Medicine, pediatrics.

Calvin M. Kunin, assistant professor, University of Virginia School of Medicine, preventive medicine.

Allan M. Lansing, lecturer (as of 1 July), University of Western Ontario Faculty of Medicine, surgery.

John A. Mannick, instructor, Medical College of Virginia School of Medicine, surgery.

Edward A. Mortimer, Jr., assistant professor, Western Reserve University School of Medicine, pediatrics.

Paul G. Quie, assistant professor, University of Minnesota Medical School, pediatrics.

Stanley L. Schrier, assistant professor, Stanford University School of Medicine, internal medicine.

Charles R. Scriver, demonstrator (as of 1 July), McGill University Faculty of Medicine, pediatrics.

William E. Shepherd, assistant professor (as of 1 July), University of British Columbia Faculty of Medicine, pathology.

Joseph C. Shipp, assistant professor, University of Florida College of Medicine, internal medicine.

Oliver Smithies, assistant professor, University of Wisconsin Medical School, medical genetics.

Ronald R. Tasker, clinical teacher (as of 1 July), University of Toronto Faculty of Medicine, neurosurgery.

James C. Warren, assistant professor, University of Kansas School of Medicine, obstetrics and gynecology.

George D. Zuidema, assistant professor, University of Michigan Medical School, surgery.

Hans Selye, professor and director, Institute of Experimental Medicine and Surgery, University of Montreal, will deliver the 40th Beaumont Lecture at the Wayne County Medical Society, Detroit, on 3 April. The subject of his lecture will be "The Pluricausal Cardiopathies."

Jean Theodore Delacour, internationally known zoologist, has been given the Addison Emery Verrill Medal for 1961 by Yale University's Peabody Museum. Delacour, recently retired director of the Los Angeles County Museum and president emeritus of the International Council for Bird Preservation, received the award at Yale early this month.

The Duke University Medical School has announced the following appointments, all effective 1 July.

Daniel C. Tosteson, associate professor of physiology at the Washington University School of Medicine (Saint Louis), has been named professor and chairman of the department of physiology to succeed **F. G. Hall**, who is retiring from the chairmanship.

John W. Moore of the National Institutes of Health has been named associate professor of physiology and chief of the school's Division of Cellular Neurophysiology.

Paul Horowicz, also of Washington University, has been named associate professor of physiology and chief of the Division of Muscle Physiology.

Lee Leiserson has joined the Directorate of Solid State Sciences of the Air Force Office of Scientific Research. He was formerly chemist with the AFOSR Directorate of Chemical Sciences.

Charles W. Lacaillade, professor of biology at St. John's University (Jamaica, N.Y.) has won the university's Outstanding Teacher Award. The first faculty member at St. John's to be so honored, he will receive the $1000 prize at the June commencement.

Book Reviews

Excellence. Can we be equal and excellent too? John W. Gardner. Harper, New York, 1961. xiv + 171 pp. $3.95.

No other writer has pleaded the case for excellence in performance in every field of endeavor with John Gardner's effectiveness and clarity. *The Pursuit of Excellence*, a report of the Rockefeller Brothers Fund, was developed under his chairmanship. Last November he was one of three members of a committee that prepared a report for the governor and the board of regents of the state of New York on meeting the demands for quality as well as quantity in that state's higher education. More recently he wrote a chapter "On education" in the report, *Goals for Americans*, prepared by the Commission on National Goals appointed by President Eisenhower. In reports from the Carnegie Corporation (of which he is president) and in many other places, he has frequently emphasized this theme. Now we are indebted to him for this small volume which summarizes his position and which should assist in clearing up fuzzy thinking about equality in a democracy.

Gardner reviews the decline of hereditary privileges and the development in America of the democratic dilemma of equality and competitive performance. He notes that these principles are present and operating to some extent in our society and that, in general, there is much confused thinking about them, particularly about equality and competitive performance. If, in the name of equality, we treat all persons as if they were equal in ability, if we permit the less capable to stifle the superior (in business, industry, civil service, trades, schools, or elsewhere), then "all who pray for the continued vitality of democracy must protest."

Gardner asks: "Is excellence possible in a democracy? Can an equalitarian society tolerate winners? Does our devotion to equality condemn us to a pervasive mediocrity? How can one possibly explain or justify the slovenliness that is openly accepted as normal in our schools, in trade unions, in industry, in government—in short, everywhere in our society? Do we honor excellences which are most fruitful for our continued vitality?" He holds that, if we can consider the answers to such questions without uneasiness, we have not thought long or hard about excellence in the United States.

In seeking solutions to the problems and conflicts inherent in these questions, Gardner appeals for a toning up of our whole society. Greatness requires high standards at all levels and for every kind of ability. It takes more than an educated elite to run a complex technological society. We "can not have islands of excellence in a sea of slovenly indifference." We must learn to honor excellence and to demand it in every socially acceptable human activity. The breadth of his approach is indicated in his statement: "We must have respect for both our plumbers and our philosophers or neither our pipes or our theories will hold water."

But the idea of excellence in itself is not universally a powerful moving force. Moving and meaningful ideas must incorporate the importance of the individual and his fulfillment. We must also have a sense of shared purposes. In spite of the diversity of our opinions and values, we do have shared aims and convictions: peace, justice, freedom, the worth of the individual, and equality before the law. All these are usually taken for granted; yet without a new sense of commitment to them, the critics of democracy may prove to be right in saying that democracy's preferences are inveterately for the inferior. Gardner does not believe this is our destiny, but the danger is real.

The role of leadership in our efforts falls heavily on the talented, many of whom refuse to accept civic responsibility, or, when they do accept it, are not always aware of the requirements of the role beyond their field of specialization. Granting that some creative persons must be allowed to be completely absorbed in their work, Gardner holds that, on the whole, gifted persons enjoy power and influence today and that our society expects power and responsible leadership to go hand in hand. "The key to a position of leadership in our society is a commitment to the highest values of the society."

Role of Institutions

Schools and colleges should be the heart of the national endeavor to contribute to the fulfillment of the individual, but Gardner urges that we change the idea that learning is only for the young and that it occurs only in school. Every institution in our society should contribute to the fulfillment of the individual. He regrets the excessive prestige attached to a college education. All young people should look forward to an active period of growth and learning throughout their lives, regardless of whether they go to college. The great variety of opportunities for learning are indicated, and the plea is made that young people not going to college be better informed about these opportunities and be given as much help and counsel as college-bound students. A person's value to society depends on continued learning, and he should be persuaded that failures and frustrations in school will not necessarily characterize his efforts in other learning situations. This position has often been stated, but never more briefly or effectively than in these few pages.

Since a democratic society grants little hereditary privilege, it places a high premium on ability. Sorting individuals on this basis is very nearly the most delicate and difficult process our society has to face. We do it in athletics, but a person's place in adult society is rarely determined by such a judgment. Our desire to protect young people from invidious comparison with respect to the more critical and important kinds of ability has produced serious confusion in educational objectives and dangerous erosion of our standards. However, as we go about the business of giving special attention to the talented, Gardner does not advocate the European system of early separation according to ability. He believes that the American principle of multiple choices, which allow successive oppor-

tunities for the individual to discover himself, must be preserved, but that it must be combined with insistence on high standards at every step. "We can not worship frivolity and expect our young people to scorn it. We can not scorn the life of the mind and expect our young people to honor it." However, he does not advance any simple answer to the perplexing problems of motivation but merely asks us to use more effectively whatever we already know about the effect of morale, environment, challenge, and stimulus.

Sheer intelligence is not proposed as the single criterion for social advancement. For one thing, bright and highly motivated persons are needed in all walks of life and at all levels. Furthermore, extreme emphasis on the intellectually gifted child might bring a reaction, as it did in the 20's, demanding equal treatment for all students. The result was to be expected in a democratic society which grants the less gifted majority the greatest ultimate control over the opportunities provided for the smaller number who can perform at the highest levels. If measures for the gifted arouse hostility, a backlash can be expected. We can best manage this particular problem by stating it in more constructive terms. "How can we provide opportunities and rewards for individuals of every degree of ability so that individuals at every level will realize their full potentialities, perform at their best, and harbor no resentment toward any other level?" In this connection, Gardner endorses the comprehensive high school in which students are grouped according to performance in each specific subject, and students of every degree of ability are placed in the home room and in extracurricular activities.

In considering individual fulfillment, Gardner maintains that the particular responsibility of our schools is to give attention to the intellectual aspects of the individual's growth. The emphasis must not be merely on teaching facts, theories, and concepts or on training in the skills but rather on instilling the proper attitudes toward growth, learning, and creativity. Learning for learning's sake is not enough, however. In addition to intellectual growth, the individual must grow emotionally and develop his character and personality. "Freedom without moral commitment is aimless and promptly self-destructive."

Gardner reminds us that a free society is still the exceptional society and that the survival of the idea for which our nation stands is not inevitable. Our fate depends on whether we, as a people, can, "despite the narcotic of easy living and the endless distractions of a well-heeled society, respond with vigor and courage and dedication to the demands that history has placed on us."

Gardner holds that our people would rather work for something they believe in than to enjoy a pampered idleness. Happiness and effectiveness are to be found in striving toward meaningful goals, including devotion to standards and respect for the human mind and spirit.

"The idea for which this nation stands will not survive if the highest goal free men can set themselves is an amiable mediocrity."

FRANK R. KILLE
State Education Department, Albany, New York

Manual of Physical Anthropology. Juan Comas. Thomas, Springfield, Ill., 1960. xxi + 775 pp. Illus. $17.50.

An Introduction to Physical Anthropology. M. F. Ashley Montagu. Thomas, Springfield, Ill., ed. 3, 1960. xvi + 771 pp. Illus. $14.50.

A Handbook of Anthropometry. M. F. Ashley Montagu. Thomas, Springfield, Ill., 1960. xi + 186 pp. Illus. $5.

These competing textbooks from a single publishing house may be likened to the offerings of some American automobile companies. Thought of in this way, Thomas can be said to have imported a successful 1957 Mexican model, the Comas *Manual*, and to have matched it with his own refurbished 1951 model, the Montagu *Introduction*. Also, Thomas has taken the unusual step of disconnecting the rear appendages of the *Introduction* and offering them unchanged as the Montagu *Handbook*, a "trailer" type of vehicle. Not surprisingly, therefore, the *Manual* looks a bit exotic in its new setting—it was designed for a different clientele, the *Introduction* shows its old lines under the added fins and chrome, and the *Handbook* is as ungainly a collection of spare parts as can be imagined. Also, judging from dimensions and price tags, Thomas has not yet changed over to the "compacts."

I will not extend this simile beyond remarking that books, like cars, are planned and produced with an eye to sales. It is up to the buyers, in this case teachers and students, to choose with care. To this end let us consider the relative merits of the *Manual* and the *Introduction*.

Having been trained in Europe and having gained experience in Mexico through years of editing and of teaching physical anthropology, Juan Comas recognized the need for a textbook in Spanish which would take into account the particular preparation of Latin American students. In 1957 he produced such a book under the title *Manual de Antropología Física* (Fondo de Cultura Economica). Its success led, after only 3 years, to the present English edition. This has not involved much change: The wise elimination of a section on statistics; amplification of chapters 2, 5, 8, and 9; and the addition of some recent references. In the above simile, in characterizing the English language edition as slightly exotic, I was not thinking of the translation, which is good, but of some of the subjects discussed, which will be unfamiliar to many North Americans: For instance, the "biotypologies" of Viola, Pende, and Barbara. Yet this does not mean that the book is in any way below the standard for introductory teaching levels in North America.

The *Manual* appeals to me because it gives historical background, strikes a good balance between the different sections, connects statements with sources by means of footnotes on the same page, does not overwhelm the beginning student with technical details beyond his need, and reports facts with a minimum of personal bias. The experienced teacher, W. M. Krogman, who contributes a preface, likewise has a high opinion of the book.

On the other hand, Ashley Montagu, who was trained in England and America and who taught anatomy in Philadelphia for a number of years, brought out the first edition of his *Introduction* in 1945 (326 pages, $4). Unfortunately, it was marred by many errors and was able to survive its bad reviews mainly because of limited competition. An improved second edition appeared in 1951 (555 pages, $8.75). The present edition is basically the second edition with the addition of quite a bit of new material. For some reason not all of the new material has been inserted in chronological order. Also, some material that

has been retained no longer deserves so much space—for example, the section on Galley Hill man (7 pages) and the excessive number of illustrations (now totaling 12) of the Swanscombe skull bones.

Having stated what I like about Comas's book, I can only say that Montagu's *Introduction* lacks these qualities. The history of the subject is restricted to 14 portraits of famous men (Boas, Yerkes, and Gregory are the American representatives). Subjects such as the primates and blood groups are given an undue amount of space, considering that they are fully covered in readily available books written by experts. The details of how to type bone and how to detemine hemoglobins and haptoglobins are beyond the scope of an introductory text. The frequent failure to indicate sources for material in the text leaves the reader in doubt about where to follow up the subject. But most of all, I object to the author's bias in reporting. This leads him to ignore much of the work of certain American anthropologists and to give undue emphasis to ideas for which he is almost the sole advocate: For example, replacing the term *race* with *ethnic group*; perpetuating the first UNESCO statement on race (largely written by Montagu) which was so unacceptable that it had to be rewritten.

On the other hand, the book has many good halftone illustrations, rather full descriptions of most of the newly discovered remains of ancient man (including Zinjanthropus, reported in 1959, but not Shanidar I, reported in 1958), and an excellent new appendix by Joseph Brožek (credited only in a footnote) on measurement of body composition. It is a shame that, after all these years, praise of the good things in this book still has to be qualified.

As for the *Handbook of Anthropometry*, I am surprised that Charles Thomas, with all his pride in fine printing, has taken to issuing separately the appendixes (that is, the most disconnected parts) of a larger book. It is true that the *Handbook* contains two sections on anthropometry, one by Montagu and the other by Brožek, but the remaining third of the book has little to do directly with anthropometry, for it contains (i) a list of anthropological periodicals (this is out of date), (ii) a section on population genetics, (iii) an impassioned plea for replacing the term *race* with *ethnic group*, (iv) the two UNESCO statements on race, and (v) the bibliography for the whole *Introduction*. The techniques for analyzing body fluids belong among these appendixes, if anywhere, but apparently they could not be separated from the text of the *Introduction*. To top all this off, Montagu has dedicated the *Handbook* to two illustrious anthropologists, Hrdlička and Matiegka, an action which I can only characterize as audacious, not only because of the things I have already mentioned, but also because these men are barely cited in the book and are no longer around to defend themselves.

T. D. STEWART

U.S. National Museum, Smithsonian Institution

Attenuated Infection. The germ theory in contemporary perspective. Harold J. Simon. Lippincott, Philadelphia, 1960. xvi + 349 pp. Illus. $10.

This book, a synthesis of clinical and epidemiological observations, laboratory experiments, and philosophical conclusions involving microbial, plant, animal, and human infection, brings down to date Theobald Smith's point of view regarding parasitism. "The germ theory in contemporary perspective" states that, although disease may occur when microbes invade tissues, or a symbiosis may result, or the invading agent may be completely eradicated, more often than not a state of equilibrium, which benefits both parties, is reached—a state "resulting in attenuated infection" or, as the author alternatively calls it, "peaceful coexistence." As he sees it, "infection refers to the presence of microorganisms within the tissues regardless of whether or not this results in detectable pathologic effects." If the host is damaged to the extent that signs and symptoms are present, the condition is infectious disease. Attenuated infection occurs either as the carrier state, as microbial persistence, or as latent infection.

Following the detailed, four-page table of contents, part 1 provides a short historical introduction, delineates the scope of the book, and defines the terms used. Part 2 deals with the general aspects and determinants of attenuated infection. Part 3 describes attenuated infection in tissue culture, and part 4 describes the same state in man. Part 5 is a brief epitome. There is an excellent bibliography of 294 titles; author and subject indexes conclude the volume. The discussions of bacteriophage, many other viruses, and tissue culture are most pertinent and certainly justify the use of the term "contemporary." The inclusion of material from the plant and animal fields adds much to the presentation of a complete and interesting picture.

Reading the book will greatly enrich the background and extend the outlook of any biologist and should be interesting and challenging to clinicians. Throughout, it is emphasized that attenuated infection is good for man. It follows that there should be mention of the effects of stress and clinical conditions—for example, diabetes—and of practices—for example, the use of antibiotics and adrenal cortical steroids—on this relationship, for some of these may disrupt the "peaceful coexistence" and bring about progressive disease.

The book would be easier to read if it had cross references to specific pages rather than to, or in addition to, sections: Thus, on page 251 we find "See section on organ differences." Search reveals this section begins on page 102. The book is well written.

In my opinion, the volume is a distinct contribution to biological literature and thought. That it has forewords by René J. Dubos and Walsh McDermott would perhaps predict such a rating.

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Ecology and Distribution of Recent Foraminifera. Fred B Phleger. Johns Hopkins Press, Baltimore, Md., 1960. viii + 297 pp. Illus. $7.50.

Ecology and Distribution of Recent Foraminifera constitutes a useful summary of the Phleger school of Foraminifera studies, for the volume is largely based on the series of publications by the author and his collaborators. Other contributions are adequately reviewed, so the volume gives a well-rounded account of recent environmental and distributional studies and of experiments with culturing Foraminifera. Although the oceanic environment is briefly characterized, there is no adequate characterization of the principal subject of the book; nonspecialists who use this volume will have to look elsewhere for a description of the organisms and for the broad outlines of classification.

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Drugs and Behavior. Leonard Uhr and James G. Miller, Eds. Wiley, New York, 1960. xix + 676 pp. $10.75.

Four years ago one of America's science reporters wrote: "Nervously, the U.S. is entering a new era—the era of the chemical modification of the human personality." In fact the use of drugs to induce sleep, to deaden the perception of pain, to produce hallucinations, and to affect behavior in numerous other ways is centuries old. The resurgence of interest in the effects of drugs upon behavior is the result of new discoveries which give promise of a possible "breakthrough" in the treatment of mental illness, discoveries which have led to the development of the interdisciplinary field of psychopharmacology. Validation of this possibility "has created an entirely new problem for drug research: the objective, scientific determination of a chemical agent's effects on human behavior and experience. To delineate and explore this problem is the purpose of this book."

To achieve this purpose the editors have organized the book in two parts: "The methodological, chemical, biological, and clinical context for psychopharmacology" and "Experimental procedures and results." The 51 chapters are the contributions of 63 authors, including the editors themselves, authors whose special qualifications lie in the disciplines of biochemistry, neurology, neurophysiology, pharmacology, psychiatry, and psychology. Some of the chapters are concerned with issues of a theoretical and systematic nature; others report specific research projects conducted by the authors.

The use of drugs to alter behavior involves interactions among three general classes of variables: drugs as chemical agents, biological systems within the living organism, and behavior patterns. *Drugs and Behavior* is primarily concerned with interactions between variables in the first and third of these classes, although some attention is given to sites in the nervous system at which drugs act and to the physiological systems affected. The text discusses the wide variety of problems which arise when attempts are made to study the interactions systematically and objectively; it describes procedures which are being used to measure the effects of drugs on animal behavior and on human behavior in both normal and pathological states.

It is a major problem to integrate the contributions of a number of different authors. In this volume the editors have done so with such success that they provide the reader with a coherent picture of an exciting area of interdisciplinary endeavor still in its preliminary stages of development. It is clear that there is much to learn about the full sensitivity of drugs used as tools for research, about their modes of action in biological systems, and about their differential effects upon behavior. Such knowledge is necessary before drugs used for the treatment of mental illness can be selected upon systematic rather than upon purely empirical grounds.

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Miscellaneous Publications

(Inquiries concerning these publications should be addressed, not to Science, *but to the publisher or agency sponsoring the publication.)*

Academic Degrees. Walter Eels and Harold A. Haswell. U.S. Office of Education, Washington 25, D.C., 1960. 324 pp. $1. This "dictionary of degrees" reports more than 1600 different academic degrees, currently conferred by U.S. colleges and universities and 800 others recognized in past years. Chapter 6, "Spurious degrees," is a brief account of weird degrees available from the degree mills; chapter 9, "Degrees arranged alphabetically," lists more than 2400 degrees, including 155 degrees (labeled "spurious") unique to the degree mills. Examples of the spurious degrees are *Diplomat of Masso-Therapy; Ortho-Geneticist; Registered Hypnotist*.

The Annual of Czechoslovak Medical Literature, 1957. National Medical Library, Prague, Czechoslovakia, 1959. 593 pp.

Asian Perspectives. Bulletin of the Far-Eastern Prehistory Assoc., vol. 3, No. 1. Wilhelm G. Solheim, Ed. Hong Kong Univ. Press, Hong Kong, 1960. 98 pp. vol. 3 (Nos. 1 and 2), $5.

Australian Atomic Energy Commission. Eighth annual report, 1959–60. Commonwealth of Australia, Canberra, 1960. 63 pp.

Collected Papers on Planetary and Space Science. Reprinted from *Journal of Geophysical Research*. Gordon J. F. MacDonald, Ed. American Geophysical Union, Washington, D.C., 1960. 82 pp. Twelve papers reprinted from the October 1960 issue; the papers were presented at the 41st annual meeting of the American Geophysical Union, sessions on planetary sciences, April 1960.

The Consumer and the New Business Cycle. 27 pp. $2. **An Action Research Program for Organization Improvement** (in Esso Standard Oil Company). 71 pp. $3. Foundation for Research on Human Behavior, P.O. Box 11, Ann Arbor, Mich.

Diseases of Wheat, Oats, Barley, and Rye. Circular 48. G. H. Boewe. Illinois Natural History Survey, Urbana, 1960. 157 pp.

Education Directory, 1960–1961. pt. 1, *Federal Government and States*. 83 pp. $0.30. **Education in the United States of America**. 144 pp. $0.50. Designed to give a broad view of education in the U.S. to persons who are not familiar with the programs and organization. **Organization and Effective Use of Advisory Committees**. Sam W. King. 85 pp. $0.30. **Studies of Home Economics in High School and Adult Education Programs, 1955–58**. Ivol Spafford. 191 pp. $0.70. U.S. Office of Education, Washington, D.C., 1960 (order from Supt. of Documents, GPO, Washington 25).

Environmental Effects on Materials and Equipment. Abstracts. vol. 1, No. 1, January 1961. Prevention of Deterioration Center. National Academy of Sciences–National Research Council, Washington 25, D.C., 1961. 40 pp. $25 per year. A monthly journal concerned with abstracting articles from the world literature. "Each issue . . . will contain at least forty substantive abstracts plus forty or more 'extracts' of current articles. . . ."

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Human Decisions in Complex Systems. Warren S. McCulloch, Ed. New York Acad. of Sciences, New York, 1961. 180 pp. vol. 89, art. 5, of the *Annals of the New York Academy of Sciences*.

The Museum Visitor. No. 1, *Survey Design*. Reports from Information Services, No. 1. D. S. Abbey and Duncan F. Cameron. Royal Ontario Museum, Toronto, Canada, 1959. 12 pp.

Nicht-Glykolytische Stoffwechselwege der Glucose. S. Hollmann. Thieme, Stuttgart, Germany, 1961 (order from Intercontinental Medical Book Corp., New York 16). 230 pp. $9.30.

Nine Glacier Maps with Text. American Geographical Soc., New York, 1960. 22 pp. + plates. 9 maps (boxed). $3. Special Publ. No. 34. Maps of eight glaciers in Alaska and one in the Olympic Mountains, Washington. Seven of the maps resulted from participation in the IGY.

Nuclear Engineering. pt. 8. Chemical Engineering Progress Symposium Series, No. 28, vol. 56, 1960. American Inst. of Chemical Engineers, New York, 1960. 83 pp. Illus.

Radiation Research in the Life Sciences. Current projects in the United States and throughout the world. Prepared for the Committee on Government Operations, U.S. Senate, and its Subcommittee on Reorganization and International Organizations. Government Printing Office, Washington, D.C., 1960. 175 pp. This report, the tenth in the subcommittee's series, outlines 3362 projects, costing over $101 million, conducted by nine U.S. agencies, ten foreign countries, and one intergovernmental organization.

Reports

Tumor-Inducing Factor in Drosophila

Abstract. Attempts to repeat experiments reported to demonstrate the existence of a tumor-inducing factor in the *tu-e* mutant of *Drosophila* were not successful.

In view of the continued references (*1*) to earlier work in this laboratory (*2*) concerning the purification and properties of a reputed tumor-inducing factor (TIF) from the *tu-e* mutant of *Drosophila*, it seems desirable to comment on the investigations described and on additional experiences.

Initially I served as adviser to F. Friedman and L. Burton on the purification of TIF with the assumption that the biological assay was satisfactory. However, shortly before the departure of these investigators from this laboratory they were presented with coded samples containing only buffer solution or buffer plus various concentrations of "purified TIF." The results of their assay demonstrated clearly that they could not distinguish buffer solution from TIF solutions. Because of the possible validity of some of the explanations offered for this, and because of some subsequent experiments of a similar nature, I undertook a repetition of tumor transmission experiments with both crude and purified extracts. Control series totaling 1047 animals, surviving to adults after injections, yielded 216 (20.4 percent) with melanotic inclusions, whereas 207 (18.5 percent) were observed among 1120 animals surviving injections of preparations from the *tu-e* mutant. Percentage survival varied widely, as did percentage "tumors" (2 to 80 percent), in different experiments, but controls did not differ significantly from experimentals when injections were made at the same time.

Since various arguments can be invoked, neither these results, nor other details not given here, prove that a tumor-inducing factor does not exist in the *tu-e* mutant of *Drosophila*. Nevertheless, I would be pleased to be forgotten as a collaborator in the work described earlier (*2*).

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2. L. Burton, F. Friedman, H. K. Mitchell, *Cancer Research* **16**, 880 (1956); F. Friedman, L. Burton, H. K. Mitchell, *Ann. N.Y. Acad. Sci.* **68**, 349 (1957); L. Burton, F. Friedman, H. K. Mitchell, *ibid.* **68**, 356 (1957); F. Friedman, L. Burton, H. K. Mitchell, *Cancer Research* **17**, 208 (1957).

2 December 1960

Rapid Effect of Sodium Cyanide and Dinitrophenol on Mammalian Nerve

Abstract. Strips of rat nerve from dorsal roots were immersed in solutions containing sodium cyanide and α-dinitrophenol. The inhibitors extinguish the action potential of such strips within minutes. The rapid action on mammalian nerve is contrasted with the slow effect of inhibitors on amphibian and invertebrate nerve.

The application of cyanide to the squid giant axone (*1*) or to frog peripheral nerve (*2*) does not block conduction or depress the resting potential for at least 2 hours. Presumably the effect of inhibitors on sheathed nerve that has been reported (*3*) may be explained as resulting from the increased leakage of ions into the extracellular space around nerve fibers; the ions having been restrained from diffusion by the envelope of sheaths surrounding the nerve fibers would become more concentrated and cause depolarization. The slow action of the inhibitors on invertebrate and frog nerve has been offered as evidence that the action potential is not directly dependent upon metabolic energy. We have attempted to assess the generality of these results by applying sodium cyanide and α-dinitrophenol to mammalian nerve.

Rats were anesthetized with ether, the lumbar roots were exposed, and thin strips of nerve were dissected from these roots. The rootlets were placed on silver stimulating and pickup electrodes; they were then stimulated with a 0.1-msec pulse, and the action potential was photographed with a Polaroid Land camera from a Dumont 304H oscilloscope after preamplification with a Grass P-4 amplifier. The strips were kept in Ringer's solution (20° to 25°C) for a 1-hour control period and immersed in Ringer's solution to which an inhibitor was added. The strips were tested periodically until the potentials disappeared; they were then placed quickly in Ringer's solution to check the reversibility of inhibition.

Strips were immersed in 0.005*M* solutions of sodium cyanide and 0.001*M* solutions of α-dinitrophenol within a *p*H range of 6.8 to 7.6. Figure 1 is a graph of the amplitude of the action potentials obtained for 17 strips immersed in cyanide (open circles) and ten strips placed in α-dinitrophenol (solid circles). The effect on individual strips may occur more rapidly than the average values indicate. The shortest disappearance of the action potential occurred in about 1 minute with a strip in cyanide solution. The longest survival time was 15 minutes. Such a range may be attributed to the variability in strip diameters.

After disappearance of the potentials, the strips were again immersed in Ringer's solution. The cyanide-poisoned nerves recovered almost the full amplitude of the action potential within 2 to 3 minutes; the strips treated with α-dinitrophenol did not recover at all, even after prolonged washing in Ringer's solution. Nerves may be treated with cyanide and reversed repeatedly, provided washing is performed immediately after disappearance of the potential.

The inhibitors cyanide and α-dinitrophenol, in contrast to their action on frog and squid nerve, rapidly extinguish the action potential of mammalian nerve. The experiments of Dettbarn and Stämpfli (*4*) have shown that α-dinitrophenol does not affect the resting potential of rat nerve. If cyanide is similarly ineffective, the inhibitors do not act by depolarizing nerve. Schoepfle and Bloom (*5*) have suggested that the effect of cyanide on frog nerve is to alter the *h* factor before altering the sodium or potassium equilibrium poten-

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

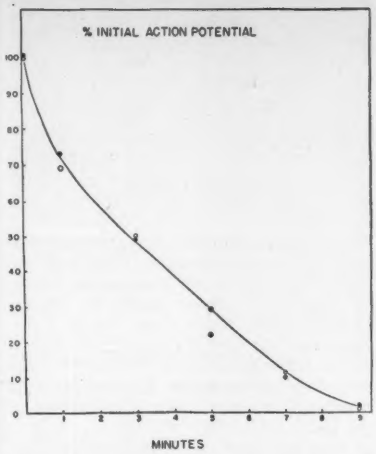

Fig. 1. Amplitude of action potentials obtained for strips immersed in cyanide and α-dinitrophenol.

tials. This mechanism of action implies that cyanide acts physically on the membrane in addition to having an effect on the cytochrome enzymes.

The inhibitors may act on mammalian nerve by blocking energy sources immediately necessary for impulse transmission. This hypothesis assumes that mammalian nerve differs from frog and squid nerve in containing a system in which the action potential is linked to the energy sources. Inhibition of the sodium extrusion mechanism, definitely dependent upon metabolism, appears to be excluded. The action of cyanide and α-dinitrophenol within minutes precludes the possibility that enough internal sodium or potassium ion accumulation occurs to depress the action potential. It is not yet possible, however, to decide whether the inhibitors act by binding physically with a membrane component or whether they act upon a metabolic system within mammalian nerve which generates the action potenial (*6*).

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30 November 1960

Self-Absorption Correction for Isotopes Emitting Weak Beta Rays

Abstract. The shape of the self-absorption correction curve of β-emitters is not universal but depends on the geometrical arrangement of the sample and counting device. This may explain why the correction factor defined by Hendler (*1*) is not always linearly related to the thickness of the sample.

In determining the radioactivity of samples of radioactive isotopes emitting weak β-rays, such as C^{14}, S^{35}, and H^3, one has to take into account that part of the radiation is absorbed in the sample itself. R. W. Hendler has described a method for calculating the self-absorption correction factor for C^{14} assay (*1*). A series of planchets of constant area containing different known quantities of the same radioactive material is prepared, and the weight m (in milligrams) and the specific count rate (in counts per minute per milligram) are recorded for each planchet. A standard weight m_{std} is chosen arbitrarily and the correction factor F (as a function of m) is defined as the ratio of the specific count rate R at weight m_{std} to the specific count rate at weight m. Hendler found experimentally that F was related to m by a linear function from infinite thinness to a weight of several times saturation thickness. This relation was contested by J. Katz (*2*), who calculated F for several sets of experimental data and found that F was not linearly related to m.

The treatment of experimental data on self-absorption is mostly based on the fact that the absorption curves of many β-emitting isotopes are approximately exponential up to a certain thickness of the absorber (*3*). If the exponential equation is applied to the set of data for $BaS^{35}O_4$, published by Katz and Golden (*4*) and quoted by Hendler in his reply (*5*), F is not found to be a linear function of m, although within a limited range of values of m the deviation from a straight line is not great. The experimental data are fitted much better by a function of m which is derived from the exponential absorption equation, provided the absorption coefficient is chosen appropriately. The parameters used for these data were $R/I_\infty = 0.0569$ and $\alpha = 0.0956$ (see below). The reference weight was 12 mg over an area of 3.14 cm^2. Within the range given by Katz and Golden ($m = 4$ to 25 mg), the deviation of the function F was smaller than 1 percent, whereas the straight line, which was suggested by Hendler (*5*), deviates by as much as 2 percent from the curve given by Katz and Golden.

From a mathematical treatment of the exponential absorption equation it follows that the slope of the curve of F plotted against m increases by a factor of two when going from $m = 0$ to $m = \infty$.

The absorption of radiation can be measured by interposing a filter of weight m over a fixed area between a sample containing a certain isotope and the detecting device. It can be seen for many isotopes that

$$I = I_0 \cdot e^{-\alpha m} \qquad (1)$$

where I is the observed count rate in counts per minute, I_0 is the count rate without filter, and α is the absorption coefficient in $milligrams^{-1}$ (*6*). This means that each layer dm of the filter decreases the radiation by the same fraction (Beer's Law). For the self-absorption it follows (*4*) that

$$I = I_\infty (1 - e^{-\alpha m}) \qquad (2)$$

where I_∞ is the observed count rate at infinite thickness. The specific count rate is I/m, and the correction factor F is defined as

$$F = (R/I)m \qquad (3)$$

where R is the specific count rate at weight m_{std}. Substituting Eq. 2 in Eq. 3 we obtain

$$F = \frac{R}{I_\infty} \cdot \frac{1}{1 - e^{-\alpha m}} \cdot m \qquad (4)$$

By definition, F is 1 for $m = m_{std}$, and

$$R = \frac{I_\infty}{m_{std}} (1 - e^{-\alpha m_{std}}) \qquad (5)$$

F can be analyzed for small values of m by expanding the exponential term into a series and by again expanding the resulting fraction into a binomial series, cutting off after the terms of the second degree:

$$F = \frac{R}{I_\infty} \cdot \frac{1}{\alpha m} \left[1 + \frac{\alpha m}{2} + \frac{\alpha^2 m^2}{12} \right] \cdot m$$

$$= \frac{R}{\alpha I_\infty} + \frac{R}{2 I_\infty} \cdot m + \frac{R \cdot \alpha}{12 I_\infty} \cdot m^2 \qquad (6)$$

It follows that the intercept of the curve of F against m on the ordinate is $R/(\alpha \cdot I_\infty)$, that the initial slope is $R/(2 \cdot I_\infty)$, and that the curve is bending upwards.

For high values of m (region of infinite thickness) the exponential term in Eq. 4 can be neglected against 1, and we get

$$F = \frac{R}{I_\infty} \cdot m \qquad (7)$$

which is a straight line through the origin with a slope of R/I_∞, that is, two times as high as at $m = 0$.

Whereas the exponential function fits very well the data of Katz and Golden (and many other sets of data published earlier), it is evident that the data cited

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by Hendler (*1*) are fitted much better by a hyperbolic function than by an exponential one. It seems thus that it depends on the counting arrangement, or possibly on the method of sample preparation, whether or not the apparent absorption coefficient, as obtained from the experimental data, is independent of the sample thickness.

It is obvious that α cannot be compared directly with the absorption coefficient of Beer's law because β-rays interact with matter in a way essentially different from electromagnetic waves. The absorption process of β-rays is a complex one, including, besides the energy losses by excitation and ionization of the absorber atoms, also scattering and backscattering, two processes which change the energy spectrum and its angular distribution (*7*).

It is therefore to be expected that it depends on the geometrical arrangement of the sample and counting device whether or not the self-absorption can be described by an exponential law with a constant α.

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3. This law is discussed already by W. Wilson, *Proc. Roy. Soc. London A* **82**, 612 (1909); see also M. Calvin *et al.*, *Isotopic Carbon* (Wiley, New York, 1949), p. 21.
4. J. Katz and S. B. Golden, *J. Lab. Clin. Med.* **50**, 658 (1959).
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16 November 1960

Action Potential and Contraction of Dionaea muscipula (Venus Flytrap)

Abstract. Observation of the action potential and contraction of the leaf of *Dionaea muscipula* Ellis revealed several interesting phenomena. Two successive stimuli are generally necessary to cause contraction. The first and ineffective stimulus is associated with slow depolarization. The second stimulus has much more rapid depolarization and initiates contraction.

The excitatory and contractile processes of *Dionaea muscipula* Ellis are of interest to biologists because of certain similarities and certain differences from the same phenomena in mammalian organs. *Dionaea muscipula* belongs to a small group of plants of the carnivorous type which are capable of trapping insects and then digesting them. The feature of interest in this study is the leaf or flytrap which contracts upon stimulation of one of the inner sensitive hairs. This plant early attracted the attention of Darwin (*1*). A complete description and pertinent references may be found in Lloyd's classical monograph (*2*). Bourdon-Sanderson (*3*) as early as 1873 recorded its electromotive properties with a capillary electrometer. Relatively little attention seems to have been paid to it until Stuhlman (*4*, *5*) recorded its action potential and characteristic of the contractile process in 1948–50 with modern methods. He showed the similarity of the *Dionaea's* action potential to that of mammalian nerve and reported on the variations to be expected from positioning of the electrodes, health and age of the plant, temperature and intensity of the stimulus. It was further shown that the action potential may run its course without producing closure of the flytrap. The present report is directed towards a further delineation of the excitatory and contractile processes.

Fresh adult healthy specimens (*6*) were obtained in early summer. They were kept in a suitable terrarium at 26°C under 12 hours of flourescent light daily. With only minor differences the method of recording the action potential was similar to that described by Stuhlman (*5*). The contraction was simultaneously recorded isometrically with a strain-gauge transducer and amplifier. Stimulation was simply done by a fine cat whisker touching one of the sensitive hairs of the inner leaf. Although the experiment was monitored with a dual-beam oscilloscope, recording was actually done with a Sanborn oscillograph. The events of excitation and contraction proved to be slow enough to be reliably recorded by this method. Only leaves with a diameter of 1.4 cm ±3 mm were used. The leaf opening usually measured 1 cm or more.

In over a hundred trials it was found that the leaf contracted upon the second stimulus. Occasionally contraction required three stimuli at 2-second intervals. Rarely did the leaf contract on the first stimulus. A typical experiment is shown in Fig. 1.

The noteworthy features are the similarity of the general contour of the action potential to that of mammalian tissues such as heart muscle (*7*). There is a rapid negative phase followed by a positve after-potential. Several minor after-oscillations may occur in some leaves. However, the item of note is that the first potential elicited showed comparatively a slower depolarization rate than the following one. This important difference is documented in Table 1, which shows the distinct shortening of the duration of the negative phase (0.13 second) of the second action potential as compared to that of the first action potential (0.24). Apparently, for the excitatory process to initiate contraction, the rate of depolarization must attain a certain velocity.

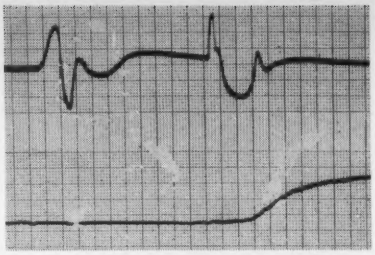

Fig. 1. Action potential and contraction of leaf of *Dionaea muscipula* Ellis. Top, action potential. Bottom, contraction. Abscissa scale (heavy lines) equals 0.2 second. Ordinate scale (heavy lines) equals 5 mv. Note that the first stimulus elicits a slow action potential which is not effective. The second action potential has much faster depolarization and causes contraction of the leaf.

From Table 1 it may also be ascertained that the positive after-potential does not show the distinct changes upon repeated stimulation as the negative phase. Not infrequently, leaves which failed to develop more rapid deplorization upon repeated stimulation also failed to contract. However, this observation was not constant, for some leaves showing characteristic action potential changes also failed to contract. Other factors which influenced the contractile process were obviously also critically important.

In the 31 leaves it was possible to determine that the mean delay between the second or effective stimulus and the onset of contraction was on the average 0.6 second (standard error, ±0.05). The time which elapsed between the onset of contraction and the development of initial tension was 1.07 seconds (average standard error, ±0.14). The attainment of maximum tension took an additional 6 to 7 seconds. By

Table 1. Comparison of the first action potential (ineffective) and second action potential (effective) of two stimuli on 31 separate leaves of *Dionaea muscipula* Ellis. Figures in the first lines under each phase are means; figures in the second lines are standard errors.

Action potential			
First		Second	
Amplitude (mv)	Duration (msec)	Amplitude (mv)	Duration (msec)
Negative phase			
11.2 ±0.8	0.24 ±0.1	14.6 ±0.7	0.13 ±0.02
Positive phase			
10.4 ±0.8	0.76 ±0.1	8.4 ±0.9	0.65 ±0.07

calibration of the strain-gauge transducer it was possible to calculate that the average force of contraction was 6.74 dy.

The ease of observation of *Dionaea muscipula* Ellis and its general availability make it a suitable object of further study of the excitation process. It should be studied with a view toward the relationship between permeability and transfer of intracellular ions with respect to its action potential.

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29 November 1960

Phylogeny of Priapulida

Abstract. The systematic position of the small invertebrate group, Priapulida, is uncertain. In more recent publications they are classified usually as pseudocoelomates in the division Aschelminthes. A histological investigation of *Priapulus caudatus*, a widely distributed species, reveals the body cavity to be a coelom, its lining a peritoneum. These features, and others, indicate the priapulids to be coelomates rather than pseudocoelomates. Unique morphology perhaps qualifies the group for the status of phylum.

In all members of the small invertebrate group Priapulida, certain basic organ systems are lacking, and other unusual structures are found. As a result, the systematic position of the group has been perhaps as uncertain as that of any group in the animal kingdom.

In the latest study of priapulid affinities, Lang (*1*) supports the popular view that the Priapulida are pseudocoelomates in the division Aschelminthes. In her treatises, *The Invertebrates*, Hyman (*2*) also places the priapulids in this position, and in a later volume (*3*) cites Lang's paper as additional evidence for inclusion of the Priapulida in the Aschelminthes.

Lang has listed numerous reasons to support his systematic conclusions. They are based in part upon his own histological investigations and in part upon those of others. He believes the more important reasons are:

(i) The dermomuscular tube of the Priapulida agrees histologically and topographically with those of the two pseudocoelomate groups, the Acanthocephala and the Kinorhyncha (Echinodera). (ii) In the Priapulida a very thin, structureless membrane, devoid of nuclei, lines the spacious body cavity. The same kind of membrane, positioned like a mesentery, holds the urogenital complex to the body wall. The membrane is structurally not a peritoneum, and thus the body cavity is a pseudocoelom. (iii) The excretory organs of priapulids, as well as of most pseudocoelomates, are protonephridia. Such organs are primitive; therefore, the body cavity they service is a pseudocoele. (iv) The proboscis apparatus is homologous in the Acanthocephala and Priapulida. (v) Kinorhynchid and priapulid nervous systems bear a striking similarity. (vi) The priapulid stereogastrula larva, first described by Lang (*1*), greatly resembles the acanthocephalan larva, and the earliest known kinorhynchid larva.

I have recently completed a histological study of *Priapulus caudatus*, one of the more widely distributed species, and have found considerable evidence indicating that the Priapulida belong with coelomates, rather than with the pseudocoelomates. The most important evidence follows:

(i) The dermomuscular tubes of both the Acanthocephala and Kinorhyncha (as well as of other pseudocoelomates) consist of syncytial tissues that exhibit relative nuclear constancy (*2*). The dermomuscular tube of *Priapulus caudatus*, however, consists of distinct tissue layers composed of discrete cells apparently of unfixed number. (ii) The membrane that lines the body cavity covers the numerous "coelomic" retractor muscles that extend between the body wall and the pharynx and covers the digestive tract and holds it to a pair of longitudinal spindle muscles. These muscles are dorsal and ventral to the gut and are free in the body cavity, except for their ends, which are attached to the pharynx and rectum. The membrane is always cytoplasmic with distinct enclosed nuclei (Fig. 1). As such it is structurally a peritoneum. (iii) Protonephridia, as found in the pseudocoelomates, are syncytial and lack nuclei in their flame bulbs, while the solenocytes of the Priapulida consists of discrete cells. (iv) The proboscis apparatus of the Acanthocephala is neither structurally nor functionally similar to the proboscis apparatus of *Priapulus caudatus*. (v) The nervous system of *Priapulus caudatus* has its main elements (circumpharyngeal nerve ring and ventral nerve cord) entirely within the body wall, in close contact with the epidermis, but distinct from it. In this way, and in others, it differs significantly from that of the Kinorhyncha. (vi) The first larval stage of both the Acanthocephala and Kinorhyncha shows extreme differences from the simple unciliated stereogastrula of *Priapulus caudatus*.

From histological examination, I have called the body cavity a coelom and its lining a peritoneum. Precise knowledge of the origin of the cavity and its lining will doubtless be revealed when embryological studies, still lacking, shall have been made. Solenocytic protonephridia are the most common form of excretory organs within the Pseudocoelomata, but they are not confined to these groups. Solenocytes are found in larval and in some adult archiannelids, polychaete worms, cephalochordates, and other true coelomates.

The presence, in these higher Meta-

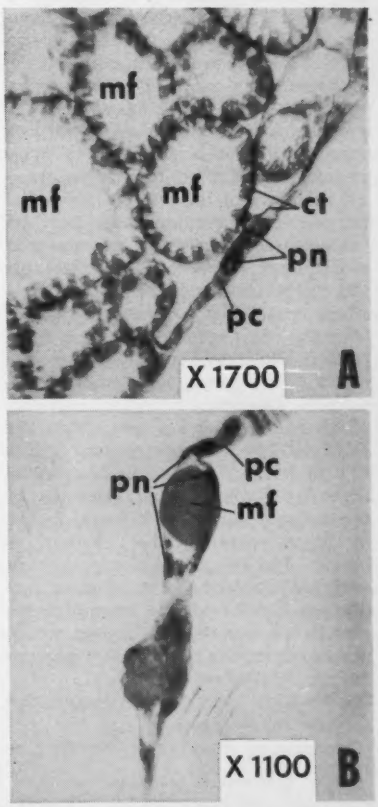

Fig. 1. (*A*) Cross section of a portion of a retractor muscle that extends between the body wall and the pharynx. Because of shrinkage the cytoplasm of the muscle fibers occupies only the peripheral portion (about × 1700). (*B*) Cross section of a portion of the mesentery between the digestive tract and a spindle muscle (about × 1100). *ct*, connective tissue; *mf*, muscle fiber; *pc*, peritoneal cytoplasm; *pn*, peritoneal nucleus.

zoa, of solenocytes structurally similar to those of *Priapulus caudatus* does not necessarily indicate a primitive organism but rather a retention of a primitive type of excretory system. To affirm homology for the proboscis apparatus of Acanthocephala and Priapulida, when embryology is unknown, is rash. The apparatus is different morphologically and functionally in both groups. The tissues present, as well as their arrangement, suggests that in the Priapulida it is probably derived, in part, from ectoderm, while in the Acanthocephala it is probably derived from mesoderm. The stereogastrula of *Priapulus caudatus* is unciliated and oval, and it consists of an inner syncytial cellular mass and an outer single layer of ectodermal cells (*1*). The priapulid stereogastrula is a simple postgastrula, while the earliest larval stage of the Acanthocephala and the Kinorhyncha are considerably more complex and advanced.

For these reasons a comparison is valueless. The larval stage of the Acanthocephala, described by Lang as possessing great resemblance to the stereogastrula, is in fact only a theoretical transitory stage in the development of the acanthor larva which does not pass through distinct blastula and gastrula stages. Simplicity of form is the greatest similarity of these three larvae, but this feature is common to the early developmental stages of all animals.

Histological evidence indicates that the Priapulida should be placed somewhere among the coelomate groups of animals. As vermiform coelomates, priapulids are unique. They possess a cuticle that is not only molted periodically through the adult life, but which has been determined chemically (*4*) and by x-ray diffraction studies (*5*) to be in part chitin. The caudal appendage, which is an extension of the coelom, and which is found in five of the six species in the "phylum", is unique. The Priapulida also possess an eversible proboscis. When all these factors are considered, the Priapulida are seen to constitute a very distinct group, perhaps deserving the status of phylum (*6*).

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Human Vigilance and Operant Behavior

Abstract. The analysis of vigilance as operant behavior treats illumination (observing) responses as operants that produce signal-detections. Evidence is presented that the relationship is an artifact of the procedure, and that no vigilance factor is involved in illumination-responses.

Human vigilance, defined by the capacity to detect rare, near-threshold signals, shows orderly changes as a function of time at work, signal-to-noise ratio, and other conditions (*1*). For the psychologist it is an aspect of the general problem of attention or alertness. It is of special interest to the human factors specialist, because vigilance is a major performance factor in monitoring displays that signal the state of malfunction of automated equipment.

Holland (*2*, *3*) proposed a technique for studying vigilance by using "observing responses"—that is, an observer's operations of a switch to illuminate the display—and showed that observing responses follow signal schedules the way other operants (*4*) follow reinforcement schedules. He also showed a good correspondence between observing response frequencies and detection frequencies. From this he concluded (*3*, p. 67) that "the detection data of vigilance studies may reflect the observing response rates generated by the particular schedules employed." The present report (*5*) tests this conclusion with a correlational analysis to determine the extent to which common factors govern variations in detection rates and observing response rates.

A paper by Baker (*6*), which appeared after the present work was completed, showed that observing responses, defined by photographic records of eye-fixations toward the display, did not behave as Holland's observing responses and were not correlated with detection rates. Rather than refuting Holland's argument, however, this result seems to us to indicate that such eye-fixations cannot be observing responses. Any reasonably defined observing response must, after all, result in a detection when it accompanies a signal. Baker's result suggests that one can appear to be looking at something without observing it. To avoid semantic confusion, Baker's responses should be called "eye-fixations," and Holland's, "illumination-responses," reserving the term "observing response" for an as-yet-unspecified act that accompanies the detection of a signal.

Signal-detections are, by definition, measures of vigilance, and our question is: are vigilance factors in signal-detections also present in illumination-responses? This can be answered simply by examining the correlation coefficients in an experiment in which the same observers perform a vigilance task twice, once with and once without the illumination-response requirement. A "vigilance" factor would be reflected in the expected significant positive correlation between signal-detections in the two performances (see *7*). If the same factor is also involved in illumination-responses, there should be a similar correlation between those responses and signal-detections in *both* performances.

The results of such an experiment are shown in Fig. 1. The apparatus used was a Mackworth-type clock (*8*) on which signals were 20° steps of the pointer that replaced 1/second 10° steps at programmed inter-signal intervals averaging 138 seconds and ranging from 52 to 203 seconds. Sixteen paid male undergraduates, working individually, monitored the clock during two uninterrupted 92-minute sessions on different days. One session required illumination-responses; in the other, the display was always visible. Order of the sessions was counterbalanced among observers.

An analysis of variance of detections for the two sessions (Fig. 1, D-1 and D-2) showed significant decrements with time and significant differences in over-all performance, but no significant order effects or other interactions. The decrements are typical of vigilance experiments (*1*), and the insignificant interactions indicate that introducing illumination-responses reduced the absolute number of detections but left the shape of the curve intact. The illumination-response curve (R-2) followed the detection curve (D-2). The latter result

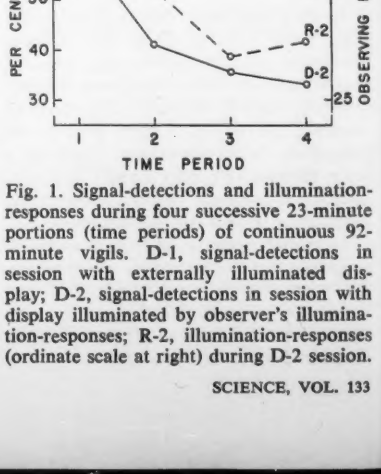

Fig. 1. Signal-detections and illumination-responses during four successive 23-minute portions (time periods) of continuous 92-minute vigils. D-1, signal-detections in session with externally illuminated display; D-2, signal-detections in session with display illuminated by observer's illumination-responses; R-2, illumination-responses (ordinate scale at right) during D-2 session.

Table 1. Spearman rank-correlation coefficients. D-1, D-2, and R-2 are defined in the legend to Fig. 1. Correlations in italics are split-half reliabilities (periods 1 + 3 versus 2 + 4).

	D-1	D-2	R-2
D-1	*.49**	.67†	.21
D-2		*.69†*	.54*
R-2			*.90†*

* Significant at the .05 level. † Significant at the .01 level.

confirms Holland's (see *3*, Fig. 9), indicating that we had succeeded in reproducing his conditions.

Using each observer's over-all performance in each session, we then performed the correlational analysis summarized in Table 1. The correlations show, first, that each measure was reasonably reliable (*9*). Second, the significant correlation between D-1 and D-2 may be interpreted as being due to the common "vigilance" factor in signal-detections. Third, the significant correlation between detections in the illumination-response session (D-2) and illumination-responses (R-2) also implies a common factor. The basic question is whether or not the latter factor is the same "vigilance" factor common to detections.

The answer lies in the correlation between detections in the session without illumination-responses (D-1) (which are governed in part by the "vigilance" factor) and the illumination-responses (R-2). The insignificant Spearman rank-correlation of 0.21 suggests that illumination-responses are not governed by a "vigilance" factor. A Kendall (*10*) partial-rank correlation, τ, is appropriate here to remove spurious correlations between D-1 and R-2 due to their common correlation with D-2. We found

$$\tau_{(D\text{-}1)(R\text{-}2).\,(D\text{-}2)} = -.09,$$

indicating the complete absence of a positive correlation and, by implication, of a "vigilance" factor in illumination-responses. Thus, except for the artifact introduced in Holland's procedure by making detections impossible unless signals and illumination-responses occur simultaneously, we cannot consider that illumination-responses govern detections, at least not detections as related to vigilance. On the other hand, Holland's results support the opposite causal relation, that detections control the rate of emitting illumination-responses.

Holland's work was inspired by the results of research on the relationship between schedules of reinforcement and operant behavior (see *4*), and it is appropriate to phrase our conclusions in the same terms. Presently available evidence permits the assumption that detections (not signals) are reinforcements. But detections are "scheduled" by the observer rather than by the experimenter, and the major problem in research on vigilance is how and why the observer produces these schedules. It is irrelevant for this problem (though certainly interesting) that the schedules, once produced, can control an operant like the illumination-response. The analogy with operant behavior is to the question of how an experimenter decides on particular schedules of reinforcement, because the observer is in the role of an experimenter arranging a schedule of detections.

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2 December 1960

Ion Uptake by Living Plant Roots

Abstract. By taking daily autoradiographs of a uniformly labeled soil in which plants are growing, patterns of actual ion uptake from the soil can be established. This technique can be used to study such influences on ion uptake as that of plant species ion diffusion, moisture and temperature stresses, and different physical, chemical, and biological properties of the soil.

Some investigators (*1*) have suggested that because of ion uptake the nutrient level is low in the soil in the vicinity of the root and a gradient exists out from the root into the surrounding soil. The exact nature of this gradient has not been established. Usually the average level of a nutrient in the soil after cropping, in conjunction with the uptake by the plant, has been used as a measure of the absorption pattern. The following technique was devised to study the actual pattern of ion uptake.

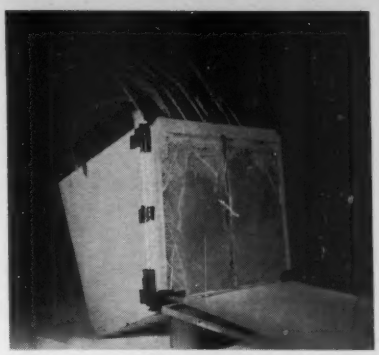

Fig. 1. Corn roots growing in uniformly labeled soil in a box designed to permit frequent taking of autoradiographs.

Corn was grown in the specially designed box shown in Fig. 1. The front side of the plywood box was sloped so that the corn roots were forced to follow this open face. A 2-mil polyethylene film was stretched across the open side, confining the soil. A ⅜-inch Plexiglas door, hinged at the bottom, could be moved up to make contact with the plastic film, or lowered, as pictured in Fig. 1, to secure the soil. A ⅛-inch layer of soil, uniformly labeled with rubidium-86, was spread next to the polyethylene film. The remainder of the box was filled with unlabeled soil. The uniformly labeled soil was prepared by stirring 100 ml of solution, containing approximately 150 μc of rubidium-86, with 250 g of air-dry 50-mesh sieved soil. After air-drying, the labeled soil was ground and mixed with a mortar and pestle.

Germinated corn was planted 1½ inches back from the polyethylene film. When the box was placed in the greenhouse, the open side was shielded from the sunlight with aluminum foil. A 1-inch layer of pearlite was placed on the top of the soil to prevent evaporation, and the soil was kept at a moisture content of approximately 20 percent.

Roots are shown growing in the labeled soil against the plastic film in Fig. 1. Autoradiographs were obtained by taking the box into a photographic darkroom and there blocking it up so that the open side would be vertical (*2*). Blocking was necessary to avoid disturbing the labeled soil. In total darkness a 10- by 12-inch no-screen x-ray film was placed on the Plexiglas door as shown in Fig. 1. The door was tightly closed, pressing the x-ray film against the polyethylene film. The film was exposed for 1 hour on the first day with an approximate 10-minute increase in exposure time daily to allow for

absorption, diffusion, and decay of the isotope.

Figure 2 is part of an autoradiograph obtained by this method. The dark background area is the uniformly labeled soil. The whiter areas are soil areas from which rubidium-86 has been removed by absorption into the plant root. The dark lines are caused by rubidium-86 concentrated in the tips and vascular systems of the roots, which has not yet been translocated to the plant top.

Because of the construction of the box, photographs, as well as autoradiographs, could be made of the roots growing in the soil. Both by visual observations of the autoradiographs and photographs and by densitometer tracings of the auotradiographs, the following conclusions have been drawn about the pattern of rubidium-86 uptake by the corn plant.

For corn from 0 to 3 weeks old, rubidium-86 absorption occurred initially through the root tip, and rubidium was translocated into the corn plant. Subsequently, continued uptake occurred all along the root, depleting the soil of rubidium-86 almost entirely in the immediate vicinity of the root. Diffusion of rubidium-86 also occurred, which replenished the absorbed rubidium-86. Diffusion was observed by cutting a root, thereby killing it, and finding that the area of absorption was refilled with rubidium-86 within 9 days.

By using the technique described in this article, it was possible to establish the actual pattern of rubidium-86 absorption from the soil by corn roots. This technique is being applied to a further investigation of the relationships between plant roots and the soil (*3*).

Fig. 2. Autoradiograph showing the removal of rubidium-86 from soil by the roots of 12-day-old corn plants (approximately two thirds of the actual size).

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5 December 1960

Coesite from Wabar Crater, near Al Hadida, Arabia

Abstract. The third natural occurrence of coesite, the high pressure polymorph of silica, is found at the Wabar meteorite crater, Arabia. The Wabar crater is about 300 feet in diameter and about 40 feet deep. It is the smallest of three craters where coesite has been found.

Since the discovery of natural coesite, the high-pressure polymorph of silica (*1*), at Meteor Crater, Arizona, the search for it in sintered materials of impact and other possible origins, such as tectonic or volcanic, has been continued in the laboratories of the U.S. Geological Survey. The work is a part of a program of crater investigations sponsored by the National Aeronautics and Space Administration. During the course of this work, a second occurrence of natural coesite was found in *suevit*, a tuff-like rock, from the Rieskessel in Bavaria, Germany (*2*). In this paper (*3*) we report a third occurrence of natural coesite, from the iron meteorite impact crater of Al Hadida.

The Wabar crater near Al Hadida (21°30′N, 50°28′E) is in a quartose sandstone of unknown age, partly buried by drift sand in east-central Arabia. It is circular in shape, and according to Philby (*4*) it is about 300 feet in diameter and about 40 feet deep. According to Spencer (*5*) large amounts of black glass (the Wabar glass), partly vesicular and partly dense, with inclusions of fractured white sandstone, are found at the crater. The presence of silica glass is also mentioned by Spencer.

Two specimens of coesite-bearing material collected from the Wabar crater by Virgil Barnes, Bureau of Economic Geology, University of Texas, have been examined in the U.S. Geological Survey laboratories. One specimen consists of white siliceous material about 1 cm across, enclosed in black glass; the second was a piece of fractured sandstone about 4 cm across (Fig. 1).

Both specimens were crushed to reduce them to individual mineral grains, and fractured quartz grains were hand-picked. These, as well as bulk specimen powders, were studied by x-ray diffraction methods. Film patterns of the bulk powders showed only one very weak reflection at 28.85 2θ (Cu Kα), which indicates the possible presence of coesite.

The x-ray film of the hand-picked, fractured quartz grains showed many more weak reflections of coesite than that of the bulk sample. We have since been able to separate relatively pure coesite and have obtained an x-ray pattern with no indication of quartz. The coesite was separated by treating 5.4 g of minus 270-mesh material with 300 ml of a water solution of 5 percent hydrofluoric acid and 5 percent nitric acid at room temperature for 3 days (coesite is considerably less soluble than quartz and glass in weak solutions of hydrofluoric acid); the product was filtered, and the treatment was repeated on the residue for an additional 2 days; then it was filtered again, and the relatively pure coesite was washed and dried.

X-ray patterns of coesite from Al Hadida agree in every detail with those of synthetic coesite and natural coesite from Meteor Crater, Arizona. Optically it is indistinguishable from the Meteor Crater coesite; it is extremely fine, with grains generally about 5μ or less. It has a mean index of refraction of 1.595 and very low birefringence.

The Wabar crater is by far the smallest of the three craters at which coesite has been found. It is estimated that the specimens from Al Hadida contain about 1 percent coesite. Because the

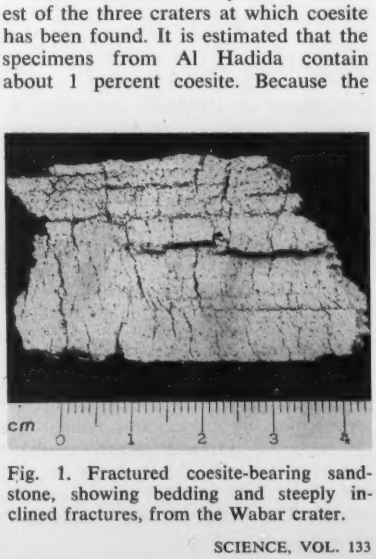

Fig. 1. Fractured coesite-bearing sandstone, showing bedding and steeply inclined fractures, from the Wabar crater.

size of a crater increases with the total energy released by impact, the presence of coesite at the Wabar crater suggests that, in siliceous rocks, impact craters of this or greater size should contain coesite. The Wabar crater is comparable in size to a crater made in alluvium by the explosion of a nuclear device at a depth of about 67 feet. This device had a yield of an equivalent of 1.2 kilotons TNT (*6*).

Natural coesite has thus far been found only in materials associated with craters of presumed impact origin where shock pressures exceeding 20 kb are thought to have occurred. We shall continue to check other possible, though unlikely, sources of coesite in deformed rocks of tectonic and volcanic origin. The evidence so far, however, supports the conclusion (*1*) that coesite is a good indicator of craters formed by impact (*7*).

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7 February 1961

Active Transport of Calcium by Rat Duodenum in vivo

Abstract. In studies of the living rat, it was observed that ionic calcium is transferred against a concentration gradient and an electropotential gradient by the duodenal membrane; this would constitute evidence for the active transport of calcium in intestinal absorption and corroborates earlier observations made in vitro. The significance of this process in regard to total calcium absorption is unknown at present.

Recently, Schachter and Rosen (*1*) have postulated the existence of an active transport system for calcium by the rat intestine; they used everted duodenal sacs in vitro. The evidence that suggested active transport was primarily (i) that calcium (labeled with Ca^{45}) was transferred against a concentration gradient and (ii) that the process was inhibited by metabolic poisons. Verification of these in vitro observations was reported by Rasmussen (*2*) in studies of the effect of the parathyroid on calcium transport, by Wasserman (*3*) in studies of the metabolic basis of calcium and strontium discrimination, and by Harrison *et al.* (*4*) in studies of the effect of vitamin D on calcium absorption. It was further shown that the degree of calcium transport was correlated with the physiological need for this ion.

Since the in vitro biological preparation is certainly nonphysiological, the argument has been put forth that observations thereon cannot be taken to mean that the active transport of calcium occurs in the intact animal. Also, in the above in vitro studies, the potential differences across the membrane were not measured. It was important, therefore, to examine in detail the kinetics of calcium transfer across the intestine of the living animal and to evaluate transport by accepted, classical procedures. The present study (*5*) was undertaken for this purpose; it was based upon the technique of Curran and Solomon (*6*).

Male albino rats (Carworth) weighing 200 to 250 g were fasted overnight before use. Surgical anesthesia was produced with sodium pentobarbital (about 6 mg/100 g body weight); the animal was maintained in this state by periodic injections of the drug. After laparotomy, an incision was made in the gastric wall, and polyethylene tubing was inserted through the stomach into the upper duodenum; the tubing was tied in place with suture. The outflow cannula and a saturated potassium chloride agar bridge were inserted into the duodenal lumen at a distance of 10 to 15 cm from the pylorus; they were then tied in place. The other agar bridge was placed in the peritoneal cavity adjacent to the duodenum. The agar bridges were led into calomel electrodes which, in turn, were attached to a sensitive electropotentiometer. The potential across the membrane was measured both directly and by determining the voltage necessary to null the measured potential. Measurements were made of the potential gradient between blood (carotid arterial and jugular venous blood), peritoneal cavity, and lumen. It was observed that the potential gradient between lumen and blood and between lumen and peritoneal cavity differed only by about 1 mv; therefore, the lumen-peritoneal potential closely approximated that of the lumen-plasma potential.

To test further the reliability of the measurement, the potential was determined when both electrodes were placed within the peritoneal cavity; here, the observed potential gradient ranged between +2 and −2 mv, indicating errors due to placement differences and differences in electrodes. Further, it was found that the potential dropped to zero when the animal was killed with one electrode in the lumen and the other electrode in either the blood stream or in the peritoneal cavity. Thus, any observed potential greater than +2 mv or less than −2 mv during the infusion studies was taken as an indication of a true potential across the membrane.

The intestine was left exteriorized and kept moistened by covering with saline-saturated absorbent cotton. The animal with attached electrodes and tubing was placed within an incubator maintained at 37°C.

The infusion solutions were made by dissolving the appropriate amount of calcium chloride (Mallinckrodt) in sterile, nonpyrogenic physiological saline. Radioactive calcium (Ca^{45}) was added at levels of 0.5 to 2.0 μc/ml. The fluids were infused in order of increasing strength at controlled rates, usually 0.0388 ml/min, by the use of a motorized syringe pump (Harvard Apparatus Co.). Before collections were made, sufficient fluid was passed through the intestine to assure that the previous solution had been completely

Table 1. Calcium fluxes across rat duodenum and a comparison of observed and theoretical flux ratios of calcium. Values for flux represent mean plus-or-minus standard error of the mean of five to six animals. Total plasma calcium was 2.6 ± 0.5 mmole, the estimated ionic plasma calcium was 1.3 mmole, and the mean potential difference between lumen and plasma was −8.1 mv.

Infused Ca (mmole)	Calcium flux (μmole/cm hr)			Ratio Ca^{++}_{lumen} / Ca^{++}_{plasma}	Flux ratio (Ca efflux/Ca influx)	
	Efflux	Influx	Net efflux		Theoretical*	Observed
0.30	0.043 ± 0.004	0.025 ± 0.003	0.018 ± 0.002	0.23	0.12	1.8 ± 0.3
0.56	.069 ± .005	.021 ± .008	.048 ± .005	.43	.23	3.2 ± .5
1.10	.11 ± .01	.034 ± .008	.076 ± .005	.85	.45	3.3 ± .9
1.97	.17 ± .02	.015 ± .004	.16 ± .01	1.52	.81	11 ± 1

* Calculated from diffusion equation of Ussing (*8*), as follows:

$$(\text{Ca efflux/Ca influx}) = (C_L/C_P) \exp ZF(\psi_L - \psi_P)/RT,$$

where C_L and C_P = concentration of ionic Ca in lumen and plasma, respectively; Z = ionic charge; R = gas constant; F = Faraday; T = absolute temperature; and $\psi_L - \psi_P$ = potential difference between lumen and plasma.

replaced. The solution flowing from the distal cannula was collected in graduated tubes for determining volume outflow per unit time. The rate of water absorption was determined by the difference between inflow and outflow rates of the solutions. Aliquots of the incoming and outgoing fluids were analyzed for calcium, sodium, and potassium by flame photometry; the radionuclide was estimated by standard radioisotopic procedures with a thin-window Geiger-Muller counter. At termination, the rats were bled by cardiac puncture. The plasma was collected and analyzed for total calcium, total protein by the biuret method, and radiocalcium.

Efflux is defined as the unidirectional movement of ion from lumen to plasma, and influx as the unidirectional movement from plasma to lumen. Net calcium flux is the difference between efflux and influx rates. The various flux rates, in micromoles per centimeter per hour, were calculated from the following equations:

$$\text{Ca efflux} = \frac{(Ca^{45}{}_i)(W_i) - (Ca^{45}{}_o)(W_o)}{[(SA_i + SA_o)/2]\,(L)} \quad (1)$$

$$\text{Net Ca flux} = \frac{(Ca^{40}{}_i)(W_i) - (Ca^{40}{}_o)(W_o)}{L} \quad (2)$$

$$\text{Ca influx} = (\text{Ca efflux}) - (\text{net Ca flux}) \quad (3)$$

The symbols are defined as follows: Ca^{45} = radiocalcium content of fluids in counts per minute per milliliter; Ca^{40} = total calcium of fluids in micromoles per milliliter; SA = specific activity of calcium in fluids in counts per minute per micromole; W = rate of water flow in milliliters per hour; and L = length of duodenal segment in centimeters. The subscripts i and o indicate inflowing and outflowing solutions, respectively. The assumptions in these calculations are that the backflow of Ca^{45} from plasma to lumen is small, and that the mean specific activity of calcium in the intestine is approximated by the average of the specific activities in the inflowing and outflowing fluids. Since the incoming and outflowing specific activities differed by only 20 percent, it was estimated that the error in using the arithmetic mean instead of the geometric mean would be small.

The essential data on the unidirectional fluxes and net transfer of calcium across the rat duodenum are presented in Table 1. First, it may be noted that the efflux and net flux of calcium increased with increasing concentration of calcium ion in the lumen; however, the influx rates were not significantly altered although there may have been a depression at the highest level of calcium (1.97 mmole). Especial note should be given to the observation that there was a net efflux of calcium at even the lowest calcium concentration. The net flux of water also was unaffected by calcium concentration or the length of time that the animal was under study. Although the data are not included here, it was further observed that the flux rates of calcium were not correlated with the net movement of sodium from lumen to plasma or with the net movement of potassium from plasma to lumen.

Total plasma calcium was about 2.6 ± 0.5 mmole (mean ± standard error of the mean) and within the normal range for the rat. The ionic calcium in plasma, as estimated from the nomograph of McLean and Urist (*7*) relating total plasma calcium, total plasma protein, and ionic plasma calcium, was found to be about 1.3 mmole, or 50 percent of the total. The potential gradient from lumen to plasma averaged about −8.1 mv; with these values and the concentration of ionic calcium in the lumen, the theoretical flux ratio was calculated from the diffusion equation of Ussing (*8*), as given in the footnote of Table 1. If the transfer of calcium from lumen to plasma and from plasma to lumen was due only to passive physicochemical forces, the observed ratio would be approximately the same as the theoretical ratio. From Table 1, it may be seen, however, that the observed flux ratio exceeded the theoretical ratio at all levels of calcium concentration in the inflowing solution. According to the usual definitions, this would constitute evidence for the active transport of calcium in the intact animal and, therefore, substantiates observations made in vitro. The contribution of this process to the total absorption of calcium by the intestine, and its importance in calcium homeostasis, is unknown at present and remains to be assessed.

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Olfactory Bulb Response of Rabbit

Abstract. An approach to understanding the properties of dendrites is to record the response of the olfactory bulb where the dendrites of mitral cells form the glomeruli. After the stimulations of the bulb and nasal mucosa, the responses appear different, but they are fundamentally composed of three successive potentials, suggesting that the last one is the action potential of glomerular dendrites.

It has been suggested that brain waves are due to the synchronization of dendritic potentials lasting 10 to 20 msec (*1*). Therefore, we studied the direct cortical response of the olfactory bulb because of its relatively simple cortical lamination. In a preliminary report (*2*) we showed that the bulb response induced by direct stimulation is composed of two successive potentials, referred to as the spike and slow potential. It seems probable that the spike potential is assigned to an action potential in olfactory nerves, the slow potential to the activity of glomerular dendrites.

Recently, Ottoson (*3*) reported that the olfactory bulb response, lasting about 150 msec, is induced by electrical stimulation of the nasal mucosa of frog and is composed of two waves, P_1 and P_2. It is thought that P_1 is a synaptic potential arising in the glomeruli, while P_2 is due to the propagated activity in secondary neurons. The present report is confined to the analysis of the potential form that can be induced by direct stimulation of the olfactory bulb and nasal mucosa.

Rabbits were anesthetized with urethan (1 g/kg), and the olfactory bulb was exposed by removal of the overlying bone and dura. Stimulation (with a 0.03- to 0.1-msec pulse) was applied bipolarly to the bulb or nasal mucosa through the hole made in the ethmoid bone. Surface recording was made from a monopolar silver-silver chloride electrode with a tip 0.1 mm in diameter. The temperature around the subject was kept at 28° to 30°C with a heating device. All recordings were made with a resistance-capacitance coupled amplifier of time constant 0.03 second. Figure 1 (*A* and *B*) demonstrates wave forms of the response recorded 1 to 2 mm from the site of stimulation, and the graded nature with increasing stimulus strength.

When it is fully developed, the response is composed of two potentials, a spike and a succeeding slow potential. The amplitude of the spike potential increases almost linearly without any detectable steps when the stimulus strength is increased. However, the slow

potential is linearly graded in the lower range of stimulus strength and reaches a plateau at about 30 volts. It can be said that these potentials behave independently of the stimulus strength. The spike reaches a summit in about 3 msec and lasts approximately 3 to 5 msec; maximum amplitude ranges from 3 to 5 mv. The slow potential shows a slow time course which lasts about 15 msec.

Sometimes the response shows different patterns owing to the ratio of amplitude between spike and slow potential. Figure 1*B* illustrates such a potential variation in which the slow potential is depressed, leaving the spike fully developed. Although the rising phase of the spike is usually rapid, in this case an inflection on the rising phase of the spike can be recorded by a fast sweep. From the experiment of twin shocks it is shown to give a summating effect, and possibly this is a sign of postsynaptic potential.

Figure 1*C* shows the record under the deteriorated state of the superficial cortex. The spike in isolation does not develop at maximal stimulation; the entire response appears as a monophasic potential. The pattern in this experiment is interpreted as due to damage and bleeding of the superficial plexuses of the olfactory nerve. Thus, it is supposed that the slow potential can be ascribed to the directly induced action potential in the secondary neuron, presumably dendrites of glomeruli, not to the transmitted activity.

As the stimulating electrode is placed on the nasal mucosa, the response induced on the bulb appears after a definite latency and has a considerably different pattern (Fig. 1*D*). The configuration and duration of the second negativity are practically the same as those of the slow potential observed at direct stimulation of the bulb. However, the initial part of the response (initial negativity) characteristically differs from the spike at bulb stimulation; its duration is about 10 to 20 msec, longer than that of the spike. In most cases the spike appears as a rising phase of the initial negativity. Therefore, it is very difficult to record the spike clearly, even by increasing the stimulus strength. This is apparently due to the asynchronous volley entering the bulb with a longer traveling distance.

It is important to determine whether the nature of such a potential is comparable to the earliest inflection and the spike observed above. To test this possibility, twin shocks at maximal strength were applied to the nasal mucosa. An interesting phenomenon observed was that the initial negativity summated when the interval was 6 to 25 msec. The main fraction of initial negativity seems to correspond with the inflection observed in Fig. 1*B*. On the contrary, the absolute refractory period measured 5 msec in the second negativity and was followed by a gradual recovery. However, in this recording the refractoriness of spike was not obvious, for the absolute refractory period is only half that of the slow potential.

The summating potential and the slow one obtained in the present work are in striking accordance with the P_1 and P_2 in the frog reported by Ottoson. If we consider it from the anatomical point of view, it will be supposed that the spike originates from the superficial plexuses of the olfactory nerve, whereas the summating and slow potentials are due to the activity of the secondary neuron. Usually the summating potential appears in a depressed or masked form in the stimulation of the olfactory bulb. On the other hand, in the stimulation of the nasal mucosa, it is very difficult to record the spike in isolation because of the asynchronous arrival of nervous impulses to the bulb. It is concluded that the response of the olfactory bulb is built up of three potentials, that is, summating, spike, and slow. Most likely, these potentials change in amplitude and form according to the stimulating sites, the stimulus strength, and the cortical excitability, but may be distinguished from one another with twin shocks.

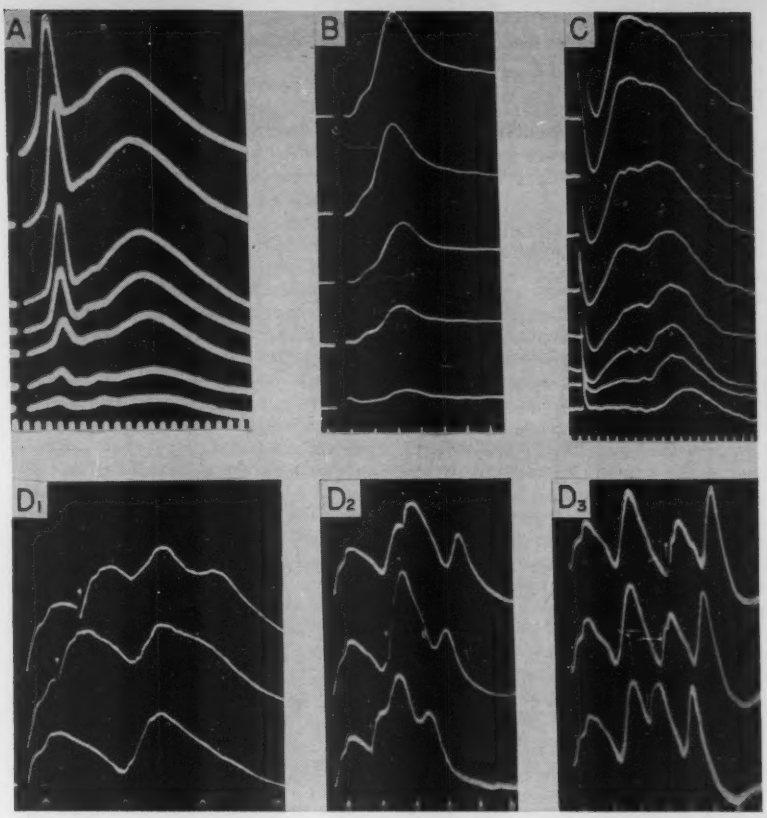

Fig. 1. Olfactory bulb response induced by direct stimulation of olfactory bulb (*A*, *B*, *C*) and nasal mucosa (*D*). In all series successive records are read from below upward. Potential was recorded monopolarly with negativity upward. *A* and *B*, Records in normal cortex; *C*, record in deteriorated cortex. Effect of increasing stimulus strength from 8 to 40 volts. Time in milliseconds. D_1, D_2, and D_3, Continuous records of twin shocks demonstrating the summation and subsequent gradual recovery. Lowest record in D_1: single response. Time interval of twin shocks: 6 to 11 msec in D_1, 16 to 32 msec in D_2, and 40 to 50 msec in D_3. Time mark, 60 cy/sec.

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12 September 1960

Preliminary Geologic Report on the 1960 U.S. Expedition to Bellingshausen Sea, Antarctica

Abstract. Thurston Peninsula, although largely snow covered, exposes massive to foliated dioritic rock and schist exhibiting northeast-trending structures. One basalt pebble was found. The adjacent continental shelf is cut by submarine valleys. Peter I Island, a dissected basaltic volcano 250 miles to the northeast, was visited.

The first geologic data from part of the little-known 1500-mile coast between Alexander I Island and the Edsel Ford Ranges (Fig. 1) were gained between 16 and 24 February 1960 when the U.S. Navy ice breakers *Glacier* and *Burton Island* penetrated the Bellingshausen Sea adjacent to the north coast of Thurston Peninsula. Landings by helicopter and launch were made by us and by Philip M. Smith, National Science Foundation representative, at seven widely separated outcrops (inset, Fig. 1).

Thurston Peninsula is covered by an undulating ice cap characterized by deep embayments and domed surfaces, 1 to 5 miles across, near the central and eastern north coast. It is likely that much of the continuous ice cap near the north coast rests on several islands rather than a deeply embayed large peninsula. The layers of the Thurston Peninsula ice cap along the coast, and the layers of the ice caps of three groups of off-shore islets, are truncated by 5- to 100-foot cliffs. In deep embayments the ice cap surface slopes smoothly to level bay ice. Apron-like terraces of ice more than 100 feet thick surround the bases of several nunataks in the northeastern part of Thurston Peninsula. Running melt water was seen or heard at most localities visited. At one locality 14 thin ice and icy-snow layers were counted in a 7-foot snow pit dug in the ice cap.

Rock exposures constitute less than 0.1 percent of the peninsula area. Exposures are found on off-shore islets, at and near the bases of a few ice cliffs, and on a few glacial horns aligned along the major axis of the peninsula. Field identification showed that the bedrock at most localities is light to medium gray, massive to faintly foliated, medium-grained amphibole-bearing dioritic rock. Thick, distinct bands of schist, traceable for several hundred feet along the strike without noticeable change, are interlayered with gneiss in the easternmost part of the peninsula. The foliation of the rocks strikes northeast and dips steeply to the southeast. Most rock is unweathered, but some limonite stains are present. Mafic dikes up to 12 feet wide are present in many outcrops. Closely spaced joints, commonly in several sets, cut all exposed bedrock. Small faults with displacements of a few inches were seen in the eastern part of the peninsula. Disintegration has produced local talus and surface rubble. Silty soil that supports mosses has developed in places in the east. Lichens partly incrust most rock surfaces.

An anomalous basalt pebble, 1-inch long, was found among gneiss debris near the eastern end of Thurston Peninsula. This pebble is subrounded and slightly faceted, and part of the surface has a dull varnish. The closest known basalt is on Peter I Island, almost 250 miles northward. Basaltic bedrock on Thurston Peninsula is probable if the pebble is a glacial eratic. R. C. Murphy, the expedition ornithologist, suggested an alternative method of transport: the pebble may be an ejected stomach stone of a sea mammal, as described from California (*1*) and New Zealand (*2*).

Significant geologic data in the vicinity of Thurston Peninsula have been contributed from oceanographic work by J. Q. Tierney, R. M. Evans, and R. B. Starr of the U.S. Navy Hydrographic Office. Echo soundings show a smoothly rising continental slope sharply set off from a nearly flat continental shelf. The shelf edge is more than 200 fathoms deep, as is common in the antarctic (*3*). Echo soundings within 5 miles of the coastline reveal an irregular topography with relief of several hundred feet. The shelf appears to be cut by several submarine valleys that probably trend perpendicular to the coast. Local terraces on the shelf may be fault controlled. Coring and dredging near the shore yielded quartzose sand, black mud, and subangular fragments of felsic and intermediate plutonic and metamorphic rock which resembles rock collected ashore. One fine-grained quartzite fragment contains a ⅛-in. smooth, rounded depression, possibly the impression of a bivalve shell. Most fragments are encrusted with modern organic forms, primarily bryozoans and worm tubes. Fragments from one station also have a gray coating. The uncoated fragments may have been recently deposited by continuing transport of rocks by ice rafting.

The age and correlation of the rocks of Thurston Peninsula are unknown. The occurrence of plutonic and metamorphic rocks on the peninsula demonstrates that the region between the Edsel Ford Ranges and the Bellingshausen Sea is not entirely a volcanic rock province, as postulated by Bentley *et al.* (*4*) from geophysical data.

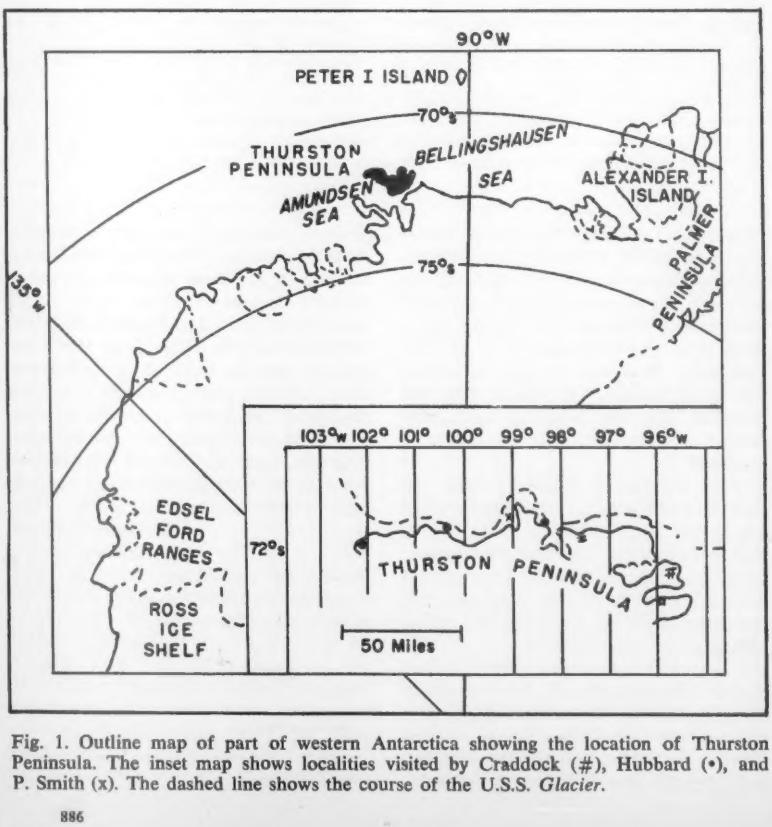

Fig. 1. Outline map of part of western Antarctica showing the location of Thurston Peninsula. The inset map shows localities visited by Craddock (#), Hubbard (•), and P. Smith (x). The dashed line shows the course of the U.S.S. *Glacier*.

Scientific work conducted at Peter I Island on 28 and 29 February and 1 March showed that the island covers less area and is higher (about 5700 feet rather than 4005 feet) than shown on U.S. Navy hydrographic chart HO-6630. Peter I Island is an extinct, deeply dissected volcano, almost entirely capped by ice; most rock is exposed on steep cliffs. At Norwegia Bay on the west side of Peter I Island, gray to dusky red, dense to vesicular basalt flows and bedded tuffs are cross-cut by basic dikes and a hypabyssal plug. The basaltic rocks contain olivine phenocrysts and mafic to intermediate inclusions (*5*).

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Distinct "Feeding" and "Hunger Motivating" Systems in the Lateral Hypothalamus of the Rat

Abstract. Electrodes were implanted in the middle hypothalamus of rats to determine the neural organization of the "feeding" centers. Stimulations of the far- and midlateral hypothalamic area produced feeding responses in sated animals, but only the former caused sated animals to cross an electrified grill to press a lever for food. After lesions had been made in the medial forebrain bundle, however, stimulations in the far-lateral hypothalamic area resulted in feeding in sated animals but failure to cross the electrical barrier to press a lever for food. Simultaneous far-lateral and "satiety" center stimulations produced feeding in sated animals but failed to "motivate" grill-crossing behavior.

The middle hypothalamus functions to regulate food intake in several animal species and has been shown to be organized into a lateral "feeding" center and a medial "satiety" center (*1*). Anand and Dua (*2*) presented evidence that the lateral "feeding" center maintains constant facilitatory influences on feeding behavior and is held in check by the more medial "satiety" region, which presumably generates inhibitory impulses in response to monitoring some circulating material indicative of the satiated state. Previous evidence (*3*) shows that the medial forebrain bundle, for which the lateral hypothalamus serves as a bed nucleus, is not the critical lateral hypothalamic system controlling basic feeding behavior, since lesions in this bundle anterior or posterior to the level of the ventromedial nuclei do not alter feeding behavior in the rat. Aphagia and adipsia result only with lesions in this bundle at the ventromedial level. Morrison, Barrnett, and Mayer (*4*) have claimed that "the medial forebrain bundle itself may be as important as the lateral hypothalamus in the control of feeding behavior," but they failed to take into account that many other systems cross the lateral hypothalamus at the level of the ventromedial nuclei. Furthermore, the lesioning method in the complexly organized lateral hypothalamus cannot possibly dissociate the medial forebrain bundle fibers from the several other trajectories, mostly pallidofugal, which enter the hypothalamus at this level. The present experiments were undertaken to fractionate functional components comprising the "feeding" center so as to ascertain the relative importance of the several systems comprising the "center" and, more particularly, to determine the possible means by which an interplay occurs between the "feeding" and "satiety" areas.

Numerous studies on feeding behavior have used a single measurement —that is, the amount of food consumed—as a determinant of "appetite," whereas in reality the essential "hunger" drive is best determined by the effort an animal will go to in order to overcome a barrier to obtain food. That certain specific "motivational" systems exist in the lateral hypothalamic area of the rat has been shown by Olds (*5*), who has found that the more general motivating properties of hunger may be produced by electrical stimulation of specific points in the brain, especially along components of the medial forebrain bundle. Since our previous studies indicate nonessentiality of this bundle in basic feeding reactions, it may well be that the medial forebrain bundle is at least important in motivating barrier crossing to obtain food, that is, as a system concerned with "hunger." Thus an attempt to study this system and its relationships with the feeding facilitatory mechanisms lying in the far-lateral portion of the middle hypothalamus comprise a part of the present study.

Adult male and female albino rats were tested for several days in a Skinner box for lever-pressing activity for food under various conditions of starvation and satiation. After several days of training for several hours a day to establish baseline lever-pressing and feeding behavior, bipolar electrodes were stereotaxically implanted in the far-lateral hypothalamic area in four animals and in the midlateral hypothalamic area in three animals. Four additional animals were given bilateral lesions in the medial forebrain bundles; then, after a testing period, electrodes were implanted in the far-lateral hypothalamic area. Three other animals had electrodes implanted in the medial forebrain bundle anterior and posterior to the level of the "feeding" centers. Finally three animals had electrodes implanted simultaneously in the far-lateral hypothalamic area and "satiety" regions. Postoperatively, after readjustment to the testing box and lever-pressing routines were set up, continuous 10-minute stimulations were carried out 20 minutes apart for 3 hours (total of six 10-minute stimulations). The stimulus parameters used were square-wave pulses of 0.2-msec duration, 60 cy/sec, at 1 to 3 volts.

Electrical stimulation of the far-lateral hypothalamic area consistently resulted in high lever-pressing rates for food and voracious feeding in satiated animals as well as "motivation" to cross an electrified grill to lever-press and feed. Stimulations in the midlateral hypothalamic area, although they often produced feeding in satiated animals, never resulted in running of the electrified "barriers" to lever-press for food. Animals with lesions in the medial forebrain bundles anterior and posterior to the level of the "feeding" centers showed no disturbances in feeding behavior. They would not feed in the sated state and never ran the electrified grill. However, after these lesions, stimulations in the far-lateral hypothalamic area still produced feeding in sated animals but no "motivation" to cross the electrical barrier to lever-press for food. Stimulations in the medial forebrain bundle itself anterior or posterior to the level of the "feeding" centers resulted neither in feeding behavior or barrier-crossing in sated animals. Simultaneous stimulations in the far-lateral hypothalamic area and "satiety" centers resulted in feeding in sated animals but consistent failure to run the electrical barrier to lever-press for food.

These data seem to indicate that the medial forebrain bundle is important in the organization of the "feeding" center as a "hunger motivational" system, since overcoming "barriers" to get to food (a measure of "hunger") depends on the essential integrity of this bundle. With this bundle interrupted, no "hunger motivation" seems to be present in

sated animals after far-lateral hypothalamic stimulation, although pure feeding responses are obtained. Thus basic feeding responses occur without the bundle but the animal will not "work" for its food. Since stimulation of the feeding and satiety areas simultaneously produces feeding in sated animals but not grill-running to lever-press for food, it seems likely that the "satiety brake" is acting on the medial forebrain bundle "motivational system" and not on the far-lateral hypothalamic basic feeding mechanisms. These results probably indicate the presence of motivational elements in the medial forebrain bundle necessary for "hunger drive" which are selectively suppressed by the "satiety" center. The far-lateral hypothalamic area would thus seem to contain the basic elements concerned directly with activation of specific feeding reflexes. It is concluded, therefore, that the "feeding" center probably is composed of both basic "feeding" and "hunger drive" elements, only the latter being depressed by the satiety mechanism (6).

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16 December 1960

Radioprotection by Mitotic Inhibitors and Mercaptoethylamine

Abstract. In the mouse, chemical interference with cellular proliferation alters the radiosensitivity of the bone marrow, and this results in protection from otherwise lethal x-irradiation. When intestinal damage is minimized by appropriate timing and dosage, many mitotic inhibitors increase radioresistance and enhance the protective effects of mercaptoethylamine.

It has been postulated that radioprotective chemicals operate by way of tissue hypoxia, inactivation of free radicals, or by the formation of mixed disulfides, or by all three (*1*). However, such mechanisms fail to explain the delayed protection against lethal radiation reported by Smith (*2*) for a colchicine derivative, or by Cole (*3*) for urethan. These latter agents have at least one common feature. That is, correct dosage results in mitotic inhibition followed by changes in cellular proliferation (*4*). With this feature as a working hypothesis, we have studied the effect of a series of mitotic inhibitors upon the radiosensitivity of mice. Preliminary results allow us to describe the action of a large class of radioprotective agents both singly and when combined with mercaptoethylamine, a known radioprotective compound.

Young female mice (Bagg Swiss), weighing 20 to 25 g, were used. Equal numbers of control mice were irradiated simultaneously with each treated group and thereafter housed jointly. Irradiations were accomplished with a G.E. Maxitron unit: 300 kv; 20 ma; HVL, 2 mm Cu; TSD, 85 cm; dose rate, 45 r/min. The 800-r dose of x-irradiation was uniformly lethal in these experiments, all control mice dying before the 21st day after exposure.

The survival data in Table 1 show clearly that mitotic inhibitors are capable of decreasing the sensitivity of mice to lethal x-irradiation. Likewise, pretreatment with these agents enhances the radioprotective effect of a small dose of mercaptoethylamine. The cytotoxic action of the mitotic inhibitors has been documented adequately by Biesele (*4*).

Most mitotic inhibitors depress both the hematopoietic tissue and the gastrointestinal epithelium, but the time for maximum depression varies for each tissue. This variation allows one to achieve selective inhibition by proper timing and optimal drug dosage. It became evident early in the course of our studies that we must avoid a combination of chemical and radio-inhibition of the intestinal epithelium. Either injury causes some degree of cell depletion and tends to increase radiosensitivity. Combined inhibition leads to severe intestinal damage which is expressed clinically by diarrhea and death 5 to 8 days after 800 r of x-irradiation (*5*). The results obtained with colcemide (*6*) (Table 1) demonstrate a biphasic effect upon radioresistance, with loss of protection 12 hours after administration of the drug. This time-effect fits the response curve for colchicine-induced inhibition of the gut as reported by Friedman (*7*).

Our experiments suggest that proper timing in the use of mitotic inhibitors will permit selective alteration of the radiosensitivity of the bone marrow. This alteration is manifested by an increase in radioresistance, and by a greater response to the protective effects of mercaptoethylamine, as measured by 30-day lethality.

Table 1. Survival data of mice receiving single large doses of mitotic inhibitors, alone and combined with mercaptoethylamine. Individual results represent groups of ten treated and ten control animals exposed to lethal x-irradiation.

Inhibitor	Intraperitoneal administration prior to x-ray		800 r 30-day survival (%)	
	Dose (mg/kg)	Time (hr)	Agent alone	Agent plus MEA*
	Metaphase inhibitors			
Colcemide	50	1	50	100
Colcemide	50	12	0	30
Colcemide	50	48	20	100
Sodium arsenite	12.5	24	40	100
Cadmium chloride	2.5	24	10	70
	Preprophase inhibitors			
Epinephrine	2	24	10	80
Urethan	1000	48	50	90
Cortisone	200	48	10	30
T-P vaccine†	†	24	10	100
	Mercaptoethylamine			
MEA	75	0.25	30	

* Mercaptoethylamine, 75 mg/kg, 15 minutes before x-ray. † Typhoid-paratyphoid vaccine (Pitman-Moore), 0.5 ml per mouse.

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8 December 1960

Meetings

Program of the Gordon Research Conferences

The Gordon Research Conferences for 1961 will be held from 12 June to 1 September at Colby Junior College, New London, N.H.; New Hampton School, New Hampton, N.H.; Kimball Union Academy, Meriden, N.H.; and Tilton School, Tilton, N.H.

Purpose. The conferences were established to stimulate research in universities, research foundations, and industrial laboratories. This purpose is achieved by an informal type of meeting consisting of scheduled lectures and discussion groups. Sufficient time is available to stimulate informal discussions among the members of each conference. Meetings are held in the morning and in the evening, Monday through Friday, with the exception of Friday evening. The afternoons are available for recreation, reading, or participation in discussion groups, as the individual desires. This type of meeting is a valuable means of disseminating information and ideas to an extent that could not be achieved through the usual channels of publication and presentation at scientific meetings. In addition, scientists in related fields become acquainted, and valuable associations are formed that often result in collaboration and cooperative efforts between different laboratories.

It is hoped that each conference will extend the frontiers of science by fostering a free and informal exchange of ideas among persons actively interested in the subjects under discussion. The purpose of the program is to bring experts up to date on the latest developments, to analyze the significance of these developments, and to provoke suggestions concerning the underlying theories and profitable methods of approach for making progress. The review of known information is not desired.

In order to protect individual rights and to promote discussion, it is an established requirement of each conference that no information presented is to be used without specific authorization of the individual making the contribution, whether it is made in formal presentation or in discussion. Scientific publications are not prepared as emanating from the conferences.

Registration and reservations. Attendance at the conferences is by application. Individuals interested in attending the conferences are requested to send their applications to the director at least two months prior to the date of the conference. *All applications must be submitted in duplicate on the standard application form, which may be obtained by writing to the office of the director.* This procedure is important because certain specific information is required in order that a fair and equitable decision on the application may be made. Attendance at each conference is limited to approximately 100 conferees.

The director will submit the applications of those requesting permission to attend a conference to the committee for that conference. This committee will review the applications and select the members in an effort to distribute the attendance as widely as possible among the various institutions and laboratories represented by the applications. A registration card will be mailed to those selected. Advance registration by mail for each conference is required and is completed on receipt of the card and a deposit of $15. (Checks are to be made payable to the Gordon Research Conferences.) The deposit of $15 will be credited against the fixed fee for the conference if the individual attends the conference for which he has applied. A registration card not accompanied by the $15 deposit will not be accepted. This advance deposit is not required for scientists from foreign countries.

The Board of Trustees of the conferences has established a fixed fee of $100 for resident conferees at each conference. This fee was established to encourage attendance for the entire conference and to increase the special fund that is available to each conference chairman for the purpose of assisting conferees who attend a conference at total or partial personal expense with their travel or subsistence expenses, or with both. This fixed fee will be charged regardless of the time a conferee attends the conference—that is, for periods of from 1 to 4½ days. It is divided as follows: registration fee, $40 ($15 for administration and $25 for the special fund); room and meals, $60 (including gratuities), for 5 days. An additional charge of $1 per night per person will be made for a room with private bath or for a single room, of which there are only a limited number. These rooms will be assigned in the order that applications are received. An additional charge will also be made for rooms occupied more than five nights.

Members attending a conference are expected to live at the conference location, because one of the objectives of the conferences is to provide a place where scientists can get together informally for discussion of scientific research of mutual interest. It is to the advantage of all participants to attend a conference for the entire week. When special circumstances warrant a request to live elsewhere, permission must be obtained from the director. If the request is approved, these nonresident conferees will be charged a registration fee of $50, instead of the resident fee of $40.

The fixed fee will cover registration, room (except room with private bath or single room), meals, and gratuities, for resident conferees. It will not provide for golf, telephone, taxi, laundry, conference photograph, or any other personal expenses.

Conferees living at the conference location who will pay all or part of the fixed fee as a personal expense may request a reduction of $25 (the amount allotted for the special fund) in the fixed fee. Application for this special fee ($75) must be made when the registration card is returned to the director.

Accommodations are available for wives who wish to accompany their husbands. All such requests should be made at the time the attendance application is submitted, because these accommodations, limited in number, will be assigned in the order that specific requests are received. Children 12 years of age and over can be accommodated at the conferences. Pets are not permitted in the dormitories.

Special fund. A special fund is provided by the Board of Trustees from the registration fee and is made available to the chairman of each conference for the purpose of increasing the participation of research scientists who could not otherwise attend or participate because of financial limitations. Its use is not limited to scientists who have been invited by the chairman to be speakers or discussion leaders. The money is to be used as an assistance fund only and may be used to contribute toward traveling expenses or subsistence expenses at the conference, or both. Total travel and subsistence expenses usually will not be provided.

Cancellations. The cancellation of an approved application for attendance at a conference will cause forfeiture of the $15 deposit.

Attendance. Requests for attendance at the conferences, or for additional information, should be addressed to W. George Parks, Director, Department of Chemistry, University of Rhode Island, Kingston, R.I. From 12 June to 1 September mail for the office of the director should be addressed to Colby Junior College, New London, N.H.

Colby Junior College

Petroleum

Cheves Walling, *chairman*
Joseph A. Chenicek, *vice chairman*

12–16 June. J. Snyder, "The chemistry of aluminum alkyls"; H. E. De La Mare, J. K. Kuchi, F. F. Rust, "Reactions between free radicals and metal ions"; J. Thomas, "Electron spin resonance studies of the mechanism of oxidation inhibition by aromatic amines"; J. C. Martin, "Solvent effects in free radical generation"; P. J. Lucchesi, "Some radiation-induced chain reactions of hydrocarbons"; H. Pines, "Reactions of carbanions with hydrocarbons." (Additional subjects to be announced.)

Catalysis

C. J. Plank, *chairman*
M. Boudart, *vice chairman*

19 June. F. S. Stone, "Photo-effects in adsorption and catalysis"; J. F. G. de la Banda, "Semi-conductivity and catalysis. The dehydrogenation of isopropanol on ZnO–Cr_2O_3 catalysts"; D. Schultze, "Electron exchange studies in carrier catalysts."

20 June. J. M. Smith, "Diffusion and heat transfer rates in porous catalysts"; D. E. Rosner, "Diffusion, heat exchange and chemical surface catalysis in external flow systems"; M. C. Freerks, "Descriptive model for the chemistry of catalyst systems."

21 June. G. E. Langlois and R. P. Sieg, "Hydrocracking of alkyl cyclohexanes"; A. Van Hook, "Carbon-14 experiments in cracking *n*-hexadecane over silica-alumina"; H. A. Benesi, "Identification of the water of constitution in hydrous oxides by infrared spectroscopy."

22 June. V. J. Frilette and P. B. Weisz, "Catalysis by crystalline aluminosilicates"; L. H. Reyerson, "Magnetic behavior of nitric oxide on hydrous oxide gels."

23 June. M. Boudart, "Application of crystal field theory to heterogeneous crystals."

Nuclear Chemistry

Ellis P. Steinberg, *chairman*
Arthur W. Fairhall, *vice chairman*

26–30 June. High-energy nuclear chemistry and nuclear cosmo-chemistry; "Present status of high-energy nuclear physics"; "Emulsion and nuclear recoil studies of high energy nuclear reactions"; "Nuclear chemistry at 25–30 Bev"; Absolute cross sections for high energy proton beam monitors"; "Mechanism of (p, 2 nucleon) reactions"; "Studies of (π,πn) and (π,πp) reactions"; "Charge distribution in high energy nuclear reactions"; "Spallation-fission competition in heavy nuclei bombarded with high energy protons"; "Extinct radioactivity: Origins of cosmic rays, meteorites, and the solar system"; "Nuclear astrophysics"; "Environmental radioactivity."

Polymers

L. A. Wall, *chairman*
F. H. Winslow, *vice chairman*

3 July. H. Mark, "Progress in polymer research"; C. R. Patrick, "Factors involved in rate coefficients in free radical polymerization"; M. Szwarc, "Reactivities of fluoro-olefins"; G. H. Crawford, D. E. Rice, B. F. Landrum, "Kinetics and mechanism of fluoronitroso polymerization."

4 July. C. C. Price, "Polyphenylene oxide polymers"; J. Eichhorn, "Synergistic agents for self-extinguishing polystyrene"; H. M. Spurlin, C. A. Lukach, D. L. Christman, "Kinetics of ethylene-propylene copolymerization."

5 July. A. D. McIntyre, "Temperature dependence of dynamic mechanical properties of mixed polymer systems"; E. G. Kontos, "Crystallizable stereoblock rubbery copolymers of α-olefins"; R. Simha, "Sequence relations in copolymers"; H. A. Hartung, "Intramolecular cyclization of network polymers."

6 July. V. Stannett, "The permeation of water through polymer films"; T. E. Davis, "Thermal degradation of polypropylene"; A. Chapiro, "Some kinetic features of graft copolymerization with special reference to radiation grafting."

7 July. P. Sigwalt, "Recent advances in block copolymerization initiated by electron transfer to monomer."

Textiles

H. J. White, *chairman*
T. F. Cooke, *vice chairman*

10 July. B. Sheldon Sprague and R. W. Singleton, "Effects of fiber structure on dyeability with disperse dyes"; Robert F. Schwenker, Jr., and Louis R. Beck, Jr., "Applications of modern thermoanalytical techniques in studies of textile materials."

11 July. O. Kratky, "Ideas and investigations on the supermolecular structure of fibrous materials"; W. O. Statton, "Order and mobility of molecules in fibers."

12 July. D. F. Arthur and C. R. Jones, "Problems of heat setting of nylon in high-speed false-twist crimping processes"; Vittorio E. Cappuccio, "Isotactic polypropylene fibers."

13 July. S. J. O'Brien and W. J. van Loo, Jr., "Chemical bonding and reaction rates of wrinkle-resistant finishes with cotton"; Mary L. Rollins, "Microscopically observed phenomena of fiber damage in cotton."

14 July. Robert E. Whitfield, Lowell A. Miller, William L. Wasley, Willie Fong, "Wool stabilization through interfacial polymerization."

Elastomers

Gerard Kraus, *chairman*
Henry Peters, *vice chairman*

17 July. F. Bueche, "Tensile properties of elastomers"; P. J. Blatz, "Application of finite elastic theory to the deformation and fracture of rubbery materials"; Gerhard Bier, "Crystalline and amorphous copolymers of olefins."

18 July. R. T. Woodhams and S. Adamek, "Unsaturated ethylene/propylene terpolymers"; C. A. Dahlquist, P. W. Trott, R. B. Althouse, "Viscoelastic properties of polyolefin elastomers"; H. J. Cantow, "Molecular structure of polybutadienes and ethylene/propylene copolymers."

19 July. E. M. Bevilacqua, "Chemistry of elastomer oxidation"; A. N. Gent, "The mechanics of ozone cracking of rubbers"; W. P. Slichter, "The study of elastomers by nuclear magnetic resonance."

20 July. David Craig and J. J. Shipman, "The conformation of alkyl and halogen substituted 1,3-butadienes"; C. E. Snyder and J. A. Lovell, "Tetrahydrofuran polymerization: mechanism and elastomeric properties"; E. K. Gladding, "A new hydrocarbon elastomer."

21 July. L. A. Walker, "The role of N,4-dinitroso-N-methylaniline in the promotion of rubber-carbon black interaction"; P. E. Wei, G. G. Wanless, John Rehner, Jr., "Reactions of isoparaffins with sulfur and di-*t*-butyl peroxide."

Corrosion: Oxidation of Metal Surfaces

Morris Cohen, *chairman*
M. A. Streicher, *vice chairman*

24 July. (H. A. Liebhafsky, *discussion leader*): E. Müller, "Corrosion studies with the field ion microscope"; A. Dravnieks, "Use of contact potential measurements in oxidation studies"; D.

Pashley, "Electron optical studies of thin surface films on metals"; J. Kruger, "The use of elliptically polarized light in the study of oxide film growth on metal surfaces immersed in aqueous solutions."

25 July. (W. Smeltzer, *discussion leader*): J. Benard and J. Oudar, "Nucleation of oxides and sulphides on metals"; T. Rhodin, "Oxide nucleation and growth on magnesium"; A. T. Gwathmey, "Some recent results on oxidation of copper and copper-nickel alloy crystals"; W. Boggs, "Oxide growth on tin and its alloys."

26 July. (A. Dravnieks, *discussion leader*): H. J. Engell, "The oxidation of metal-carbon alloys"; C. E. Birchenall, "Some problems in high temperature oxidation of metals"; E. A. Gulbransen, "The effects of environment, stress, and metal structure on the oxidation of iron and iron alloys"; J. Laukonis, "Formation and reduction of oxide films on iron whisker surfaces."

27 July. (M. A. Streicher, *discussion leader*): M. Dignam, "Anodic oxidation of aluminum—transient effects and oxide structure"; R. A. Baker, "Anodic oxidation of lead in sulphuric acid"; M. Cohen, "A comparison between air-formed and anodic oxide films on iron."

28 July. (M. Cohen, *discussion leader*): R. C. Plumb, "Electrode potential transients and oxide films"; H. J. Engell, "Breakdown of oxide films and pitting."

General discussion.

Medicinal Chemistry

C. K. Cain, *chairman*
W. M. Govier, *vice chairman*

31 July. Symposium: *Antiviral chemotherapy*. Ernest C. Herrmann, "Problems in screening and evaluation of antiviral substances"; Igor Tamm, "Selective inhibition of viral multiplication and cytopathogenicity by benzimidazole derivatives"; P. W. Sadler, "Development of antiviral agents"; D. J. Bauer, "The chemotherapy of the pox diseases."

1 Aug. Domingo M. Aviado, "Pharmacology of the pulmonary circulation"; Andrew L. Banyai, "Shortcomings of medicinal therapy in lung diseases and their possible correction"; Ernest L. Dobson, "Homeostatic control of body fluid volume."

2 Aug. Amino acid analogs as decarboxylase inhibitors. Karl Pfister, Sidney Udenfriend, Clement A. Stone (subjects to be announced); Benjamin Zweifach, "The role of the reticuloendothelial system in adaptation to injury."

3 Aug. Chlordiazepoxide (Librium), a new psychotherapeutic drug. Leo Sternbach, "Chemistry and metabolism"; George Heise, "Pharmacology and behavior analysis in animals"; Ebbe C. Hoff, "Clinical experience"; Morton F. Mason, "Some problems in analytical toxicology imposed by modern development in drugs"; Ernest E. Muirhead, "The antihypertensive function of the renal medulla."

4 Aug. Ivan Mota, "Studies of histamine liberation and the effect thereon of nicotinic acid derivatives"; Donald B. McCormick, "Phosphorylation of vitamin B_6 in brain and its inhibition by carbonyl reagents."

Qualified scientists are invited to submit applications for attendance at the Gordon Research Conferences. Application blanks may be obtained by returning the coupon that appears on page 898 to Dr. W. George Parks, Department of Chemistry, University of Rhode Island, Kingston, R.I.

Separation and Purification

Harry P. Gregor, *chairman*
J. Ward Greiner, *vice chairman*

7 Aug. L. C. Craig, "Membrane purification techniques"; I. A. Eldib, "Zone precipitation"; Dieter Gruen, "Solvent extraction of actinides and lanthanides from fused lithium nitrate–potassium nitrate eutectics."

8 Aug. W. Schlenk, "Formation of clathrate compounds as a basis for separative procedures"; A. J. Barduhn, "Properties of some new gas hydrates and their use in separation processes"; W. G. Knox, "Large scale separations by gas hydrate formation."

9 Aug. W. Groth, "Separations by the gas centrifuge"; W. J. A. Vanden Heuvel, "Gas chromatographic separations of steroids and other high molecular weight compounds"; Gerson Kegeles and J. L. Bethune, "Countercurrent distribution of chemically reacting systems"; A. A. Rosen, "Adsorption and elution processes with charcoals."

10 Aug. Discussion: *Applications of separation and purification procedures in the pharmaceutical industry*. Kotaro Murai, "Purity criteria and applications of countercurrent distribution"; Charles Pidacks, "Partition column chromatography—A versatile laboratory scale method of separation and purification"; S. A. Fusari, "The role of complex formation in the separation and purification of antibiotics"; L. J. Heuser, "Modification by formation of chemical derivatives for separative procedures."

11 Aug. J. E. Cochran, "Large scale column chromatography of *Vinca Rosea* Linn. alkaloids"; R. D. Walker, Jr., "Heights of transfer units in adsorption fractionation."

Instrumentation

J. M. Vandenbelt, *chairman*
Elwyn D. Jones, *vice chairman*

14–18 Aug. D. R. Herriott, "Characteristics of optical maser beams and some measurement applications"; W. H. Straub, "Methods of optical communication"; K. M. Baird and D. S. Smith, "Very precise electronic setting on interferometric fringes"; H. H. Plumb, "The acoustical interferometer as an instrument for determining absolute temperatures at 4°K"; Robert A. Woodson, "The fringe-count micrometer and automatic long path interferometer"; Martin E. Packard, "The present state of the art and future trends of NMR and EPR"; W. Reddish, "Refinements of technique for precise measurement of dielectric constant and power factor of solid insulating materials"; R. J. Meltzer, "Design considerations for spectrophotometric color measurement"; A. F. Sperry, "On-line computers for chemical processes"; William H. Surber, "Adaptive control systems"; A. J. Young, "Current approach to process control in Britain"; A. L. G. Rees, "Scientific instrument developments in Australia"; John F. Davis, "The mechanisms of information transfer and computation in the process control system in man"; Manfred Clynes, "The first bio-cybernetic law: unidirectional rate sensitivity"; Lawrence Stark, "The pupillary servo mechanism"; John Atwood, "Biological noise in the human pupil"; Marcus O'Day, "Electric propulsion of space vehicles"; David H. Fuller, "Evaluation of gas composition detectors"; L. W. Zabel, "A paper basis weight uniformity instrument"; J. E. McCarley, "An instrument for measuring viscoelastic properties at low rates of shear"; Raymond Jonnard, "Sampling theorem and the speeding up of automatic chemical analysis"; F. D. Ezekial, "Recent developments in torque motor design and applications"; S. Y. Lee (subject to be announced).

Food and Nutrition

J. C. Bauernfeind, *chairman*
D. M. Hegsted, *vice chairman*

21 Aug. Food additives (Bernard L. Oser, *chairman*): Bernard L. Oser, "Food additives, status and problems"; Julius Coon, "Toxic substances in natural foods and their significance"; J. Raymond McMahan, "Food antimi-

crobial agents, technology and legality." *Special nutrition problems* (Herbert Pollack, *chairman*): Doris H. Calloway, "Food problems of the armed forces, present and future"; Albert Taylor, "Progress in space foods and nutrition."

22 Aug. Fatty acids and tocopherol (Robert Harris, *chairman*): Fred A. Kummerow, "Fatty acid interrelationships in lipoprotein"; Max Horwitt, "Polyunsaturated fatty acids and tocopherol"; A. L. Tappel, "Selenium, tocopherol, and antioxidant function." *Carbohydrates and nutrient utilization* (Z. I. Kertesz, *chairman*): Albert Renold, "Comparative metabolism of carbohydrates in normal and diabetic individuals"; Willard A. Krehl, "Utilization of nutrients as affected by food composition, food processing, and nutrient interrelationships."

23 Aug. Nutrition and disease (Nevin S. Scrimshaw, *chairman*): Nevin S. Scrimshaw, "Nutrition and natural resistance to disease"; Robert E. Hodges, "Nutrition, acquired immunity and antibody formation"; William B. Dewitt, "Nutrition and helminthic infection"; Walter E. Knox, "Nutrition and metabolic diseases"; Benjamin Pasamanick, "Nutrition and mental disease."

24 Aug. Nutrition and disease (continued): Roger Maickel, "Nutrition and drug toxicity or metabolism"; Elmer L. Severinghaus, "Nutrient supplements and hospital diets as adjuvants of medical care." *Future food problems* (Paul E. Pearson, *moderator*): *The exploding population, the food problem, and action plans to meet the challenge* (a three-member panel program).

25 Aug. Obesity and atherosclerosis (Theodore Van Itallie, *chairman*): Theodore Van Itallie, "Dietary, depot, and intimal fat": Albert J. Stunkard, "Dietary intake, activity, and obesity"; Laurance W. Kinsell, "Nutritional advances in atherosclerosis."

Cancer

Chester M. Southam, *chairman*
Sidney Weinhouse, *vice chairman*

28–29 Aug. The complex etiology of cancer: L. Gross, H. Rubin, R. Dulbecco, K. DeOme, "Virus phenomena"; K. DeOme, L. Gross, M. Duran-Reynals, "Interactions of viruses and other factors"; P. Kotin, A. W. Horton, J. Orr, R. S. Schwartz, "Interactions of factors other than viruses"; A. E. Moore, J. Grace, S. O. Schwartz, S. Stewart, "The search for cancer viruses of man."

30–31 Aug. Interrelationships of tumor and host: R. Golbey, G. Foley, R. R. Smith, N. Molomut, "Vagaries of tumor growth"; S. Albert, D. Clarke, "Non-antibody host factors"; L. Ortega, S. Lawrence, R. S. Schwartz, "Mechanisms of tissue rejection"; G. Klein and J. Finney, "Cancer specific antigens."

1 Sept. Fine structure of normal and cancer cells: C. T. Ashworth, E. Essner, G. C. Easty, K. Porter, R. C. Buck.

New Hampton School

Chemistry of Coal

Richard A. Glenn, *chairman*
L. L. Newman, *vice chairman*

Science in the Preparation of Coal

12 June. W. A. White, H. F. Yancey, C. C. Harris, "Behavior of clays during washing"; D. A. Hall, C. C. Harris, E. R. Palowitch, "Dewatering and drying of coal."

13 June. R. P. Gardner, R. L. Brown, J. A. Harrison, "Breakage of coal"; H. B. Charmbury and R. A. Glenn, "Selective pulverization."

14 June. R. L. Brown, D. A. Hall, J. R. Garvey, "Flow of coal"; N. Berkowitz, "Coal-oil slurries for pipeline transport"; J. L. Bayer, H. L. Benedict, "Effects of preparation on petrographic composition."

15 June. I. N. Plaksine and V. I. Klassen, "Froth flotation processes"; I. N. Plaksine and N. F. Olofinsky, "Electrostatic cleaning"; E. R. Palowitch, "Thermomagnetic separation of pyrite"; M. R. Geer, A. W. Deurbrouck, "Dense-medium cyclone desister table."

16 June. D. A. Hall, A. M. Wandless, P. G. Meerman, "Quality control"; L. L. Newman, "Coal Gasification." Planning for next conference.

Chemistry and Physics of Liquids

R. H. Cole, *chairman*
Walter H. Stockmayer, *vice chairman*

19–23 June. B. J. Alder, "Machine studies of molecular dynamics"; O. K. Rice, "Thermodynamics and statistical mechanics of non-uniform fluids"; Z. W. Salsburg, "Cell theories for solutions of molecules of different sizes"; R. L. Scott, "Thermodynamic properties of solutions"; J. S. Rowlinson, "Problems of the precise interpretation of the properties of simple mixtures in terms of intermolecular parameters"; J. H. Hildebrand, "Forces between molecules of different species"; J. E. Mayer, "Equations for entropy production in transport"; H. Eyring, "Extended free volume theory of transport processes"; D. C. Douglass, "Self-diffusion and the velocity correlation function"; A. R. Ubbelohde, "Cluster theory of viscosity, prefreezing in relation to premelting"; J. Lamb, "Viscoelastic and ultrasonic relaxation"; R. H. Cole, "Dielectric relaxation in liquids"; W. DeMaeyer, "Studies of chemical kinetics in liquids by relaxation methods.

Proteins

J. T. Edsall, *chairman*
Leon Heppel, *co-vice chairman*
Cyrus Levinthal, *co-vice chairman*

26–30 June. New developments in protein analysis, separation of peptides, and peptide chain sequences. Three-dimensional structure of proteins—the evidence from x-ray diffraction, optical rotation, deuterium exchange, and so on. (This will include discussion of unfolding and refolding of protein molecules in different solvents). *Structural basis of enzyme specificity* (insofar as this can be now discussed in terms of protein structure.) Among those taking part will be: C. B. Anfinsen, Jr., G. Braunitzer, L. C. Craig, F. R. N. Gurd, V. M. Ingram, E. Katchalski, J. C. Kendrew, I. M. Klotz, B. W. Low, S. Moore, H. Neurath, M. F. Perutz, F. M. Richards, J. A. Schellman, H. A. Scheraga, W. A. Schroeder, W. H. Stein, C. Tanford, and B. L. Vallee.

Scientific Information Problems in Research

Robert A. Harte, *chairman*
Karl F. Heumann, *vice chairman*

3 July. Theory of information retrieval (Calvin N. Mooers, *session leader*). *The abstracting problem* (G. Miles Conrad, *session leader*): Milton O. Lee.

4 July. The indexing problem (Charles L. Bernier, *session leader*): E. J. Crane; Frank B. Rogers. *The retrieval problem* (Saul Herner, *session leader*): Robert L. Hayne.

5 July. Character recognition devices (Paul W. Howerton, *session leader*): Harry J. Woll; John A. Fitzmaurice; Charles W. Allen. *The hardware problem* (Ascher Opler, *session leader*): Eugene Wall; Jack Minker.

6 July. Special services (C. Dake Gull, *session leader*): Burton W. Adkinson, "The role of government."

7 July. Goals and prospects (session leader to be announced).

Magnetic Resonance

G. E. Pake, *chairman*
B. P. Dailey, *vice chairman*

10 July. (N. Bloembergen, *chairman*): G. Benedek, "Temperature and pressure dependence of chemical shifts in octahedral cobalt complexes"; W. M. Walsh, "Effects of pressure and temperature on crystalline field splittings." (H. S. Jarrett, *chairman*): D. I. Bolef, "Acoustic absorption techniques in NMR"; R. G. Shulman, "Knight shifts in intermetallic compounds and alloys"; V. Jaccarino, "NMR properties of intermetallic compounds."

11 July. (F. Bloch, *chairman*): C. D. Jeffries, "Dynamic nuclear orientation"; A. Abragam, "Dynamic polarization

and spin temperature." (H. C. Torrey, *chairman*): C. Ryter, "Measurement of electron densities by observation of the day shift"; J. Lambe, "ENDOR mechanisms in solids."

12 July. (R. E. Norberg, *chairman*): J. I. Budnick, "NMR in ferromagnets"; M. Weger, "Nuclear relaxation processes in ferromagnets." (D. W. McCall, *chairman*): E. R. Andrew, "NMR experiments with rapidly rotating crystals"; J. Powles, "Magnetic relaxation in polymers."

13 July. (H. S. Gutowsky, *chairman*): S. I. Weissman, "Possible new biradicals"; D. B. Chesnut, "ERP studies of spin correlation in some ion-radical salts"; H. N. McConnell, "Rates of intramolecular charge- and spin-exchange in aromatic radicals." (W. Gordy, *chairman*): D. H. Whiffen, "Electron resonance spectra of some non-aromatic organic free radicals"; R. W. Fessenden, "ERP studies of transient radicals."

14 July. (C. A. Hutchison, Jr., *chairman*): J. H. van der Waals, "Investigation of phosphorescence of aromatic hydrocarbons by electron resonance"; G. W. Ludwig, "Study of impurities in semi-conductors by spin resonance."

Radiation Chemistry

John L. Magee, *chairman*
Ellison H. Taylor, *vice chairman*

17 July. (W. M. Garrison, *chairman*): M. Burton, "Present status of radiation chemistry." (C. J. Hochanadel, *chairman*): F. S. Dainton, "Radiation chemistry of water and aqueous systems. Status of experimental work."

18 July. (P. J. Dyne, *chairman*): A. Kuppermann, "Radiation chemistry of water and aqueous systems. Status of theory." (H. A. Schwarz, *chairman*): L. Dorfman, "Observation of intermediates in radiation chemistry."

19 July. (F. W. Lampe, *chairman*): "Impact processes and ion-molecule reactions" (speaker to be announced). (E. H. Taylor, *chairman*): contributed papers on recent work.

20 July. (M. Magat, *chairman*): S. Lipsky, "Energy transfer." (H. A. Dewhurst, *chairman*): A. Chapiro, "Radiation chemistry of polymerization."

21 July. (L. Wall, *chairman*): M. A. Golub, "Radiation chemistry of polymers."

Organic Reactions and Processes

Harold E. Zaugg, *chairman*
William A. Mosher, *chairman-elect*

24 July. Erwin Klingsberg, "The 1, 2-dithiolium cation: a new pseudoaromatic system"; Stanley J. Cristol, "Reactions of anthracenes and dihydroanthracenes."

25 July. W. J. Pummer, L. A. Wall, J. E. Fearn, J. M. Antonucci, "Reactions of aromatic fluorocarbons"; C. David Gutsche, "Intramolecular reactions of diazoalkanes"; G. A. Olah, "Newer methods and investigation of the mechanism of Friedel-Crafts type reactions."

26 July. Christian S. Rondestvedt, Jr., "The catalytic isomerization of *m*-dioxanes to β-alkoxy aldehydes"; R. H. Hasek, "Dialkylketenes"; Rolf Huisgen, "1, 3-Dipolar additions."

27 July. R. F. Heck and David S. Breslow, "The mechanism of the oxo reaction"; Hugh L. Dryden, Jr., Gayle M. Webber, Robert R. Burtner, John A. Cella, "The use of sodium in the birch reduction of aromatic compounds"; Norman H. Cromwell, "Polycyclic heterocyclic syntheses. Benzacridines and indenoquinolines."

28 July. Robert L. Letsinger, "Organoboron-nitrogen compounds. Chemical and catalytic properties."

Steroids and Other Natural Products

G. Büchi, *chairman*

31 July–4 Aug. P. Beal, "Chemical modification of the corticosteroids"; K. Biemann, "Applications of mass spectrometry in the determination of the structure of natural products"; L. Crombie, P. de Mayo (subjects to be announced); R. E. Ireland, "Syntheses in the terpene field"; E. Leete, "Recent developments in the biogenesis of natural products"; N. Leonard, "The chemistry of triacanthine"; H. Musso, "Orcein, litmus, and some aspects of phenol oxidation"; K. Nakanishi (subject to be announced); A. Patchett, "A new class of aldosterone antagonists"; H. Rapoport, H. Schmid, W. I. Taylor (subjects to be announced); H. Wassermann, "Bacterial pigments."

Statistics in Chemistry and Chemical Engineering

Carl A. Bennett, *chairman*
J. Stuart Hunter, *vice chairman*

7 Aug. J. S. Hunter and Cuthbert Daniel, "Screening experiments for factors"; C. W. Dunnett, "Screening experiments for treatments."

8 Aug. J. W. Gorman and P. W. M. John, "Experiments with mixtures"; R. W. Sommers, "Critical factors in the selection of theoretical and empirical models."

9 Aug. Rutherford Aris, "Chemical engineering applications of spectral analysis"; Gwilym M. Jenkins, "Spectral approach to industrial experimentation."

10 Aug. A. J. Duncan, "Bulk sampling"; Edgar Anderson, "Semigraphical methods of analysis."

11 Aug. Marvin Zelen, "Robustness of life testing procedures."

Analytical Chemistry

Sidney Siggia, *chairman*
Donald D. DeFord, *vice chairman*

14 Aug. C. B. Murphy, "Differential thermal analysis"; D. J. Fraade, "Continuous analysis."

15 Aug. I. Frankuchen, "The application of x-rays to structure determination of organic materials"; L. S. Birks, "The electron probe."

16 Aug. H. Fischbach, J. W. Cook, L. L. Ramsey, "Analysis and food and drug regulations"; W. W. Bates, "Analysis and composition of tobacco smoke."

17 Aug. J. E. Lovelock, "Electron affinity spectroscopy." (This will be an open session.)

18 Aug. G. A. Harlow, "New developments in the determination of acids and bases."

Inorganic Chemistry

H. M. Haendler, *chairman*
M. L. Nielsen, *vice chairman*

21 Aug. The metal-oxygen bond (D. C. Bradley, *chairman*): L. G. Sillen, "Metal-oxygen hydrolytic polymer systems"; J. Lewis, "Magnetic interaction through M-O-M systems"; M. C. R. Symons, "Structure and reactivity of oxy-anions of transition metals"; C. C. Addison, "Covalent metal nitrates."

22 Aug. F. A. Cotton, "Metal-oxygen chelate complexes"; R. C. Mehrotra, "Metal alkoxides and metal carboxylates"; P. J. Wheatley, "X-ray studies on alkali-metal alkoxides"; C. N. Caughlan, "X-ray studies on titanium alkoxides"; D. C. Bradley, "Structural aspects of metal oxide alkoxides."

23 Aug. Fluorine chemistry (K. Knox and R. W. Mason, *cochairmen*): C. P. Colburn, "Current developments in the nitrogen-fluorine field"; J. S. MacKenzie, "New developments in oxygen-fluorine chemistry"; G. L. Goodman, "Electronic structure of metal hexafluorides"; M. Schmeisser (subject to be announced); F. Seel, "Coordination compounds of nitrogen and halogen fluorides"; E. L. Muetterties, "Stereochemistry of metal fluorides and their complexes."

24 Aug. R. D. Peacock, "Recent work on the fluorine compounds of the transition metals"; R. Hoppe, "Preparation and crystal chemistry of some complex fluorides of the transition elements and the rare earths"; K. Knox, "Structures and properties of some complex fluorides."

25 Aug. V. Gutmann, "Some molecular addition compounds and their reactions in solution"; R. Herber, "Radiochemical evidence for inorganic molecular complexes"; N. N. Greenwood, "Thermochemistry of molecular addition compounds."

Adhesion

Robert L. Patrick, *chairman*
F. H. Wetzel, *chairman-elect*

28 Aug. Turner Alfrey, "Bulk properties of polymers and their effect on adhesion"; Edward J. Ripling, "Bulk properties of metals and their effect on adhesion."

29 Aug. Peter Debye, "Molecular forces and zero-point energies"; George G. Cocks, "The effect of molecular structure and shape on adhesion."

30 Aug. William C. Wake, "Rubber-textile adhesion." Panel discussion: *Fiber rubber adhesion* (H. T. Patterson, Frank N. Kelley, M. W. Wilson).

31 Aug. G. R. Sprengling, "Curing mechanisms of phenolic-elastomer blends"; Louis H. Sharpe, "Surface analysis by infrared—new techniques"; Charles Blaich, "Elastomeric properties in adhesion."

1 Sept. Leonard Suffredini, "Glass flake reinforcement and flaw theory."

Kimball Union Academy

Lipide Metabolism

Jules Hirsch, *chairman*
Maurice Rapport, *vice chairman*

12 June. (J. David Robertson, *chairman*): V. Luzzati, "The structure of liquid-crystalline phases in association colloids: description of lipid-water systems"; W. Stoeckenius, "The electron microscopy of lipid and lipoprotein structures." (Sanford L. Palay, *chairman*): Jack H. Schulman, "Physical chemistry of the absorption of emulsions in living systems"; C. T. Ashworth, "Cellular dynamics in intestinal absorption."

13 June. (E. B. Astwood, *chairman*): E. B. Astwood, "Pituitary hormones and fat mobilization"; T. M. Chalmers, "A lipid-mobilizing agent from human urine"; Daniel Rudman, "Studies on a lipid-mobilizing substance of the pituitary gland." (James A. F. Stevenson, *chairman*): James A. F. Stevenson, "The hypothalamus and the regulation of energy and water exchange"; Irving L. Schwartz, "Chemoreceptor mechanisms in the regulation of food intake"; Albert F. Debons, "Brain and tissue distribution of gold thioglucose and gold thiomalate"; Donald C. Borg, "Some advantages and limitations of radioactivation as a method for elemental analysis."

14 June. (E. C. Horning, *chairman*): Donald C. Damoth, "The fundamentals of mass spectrometric analysis"; Einar Stenhagen, "Mass spectrometric structure determination of fatty acids"; C. A. Genge, "Application of mass spectrometry to lipid biochemistry." (E. C. Horning, *chairman*): T. J. Flautt, "Structural analysis by means of high-resolution nuclear magnetic resonance"; C. Y. Hopkins, "The utility of nuclear magnetic resonance in the analysis of glycerides and fatty acids"; E. C. Horning, "Gas chromatographic separations of steroids and related compounds."

15 June. (Lowell E. Hokin, *chairman*): Lowell E. Hokin, "The phosphatidic acid cycle and sodium transport"; R. A. Pieringer, "The biosynthesis of lysophosphatidic acids from monoglycerides and ATP"; C. G. Huggins, "Phosphorus metabolism in phospholipid and phosphatidopeptide as related to denervation and supersensitivity in the submandibular salivary glands"; F. N. LeBaron and George Hauser, "The metabolism and possible functional role of brain phosphoinositides and their complexes with proteins." (Leonard B. Kirschner and M. L. Karnovsky, special discussants.)

16 June. (E. H. Ahrens, Jr., *chairman*): John Senior, "Studies of fatty acid metabolism and absorption in the intestinal mucosa"; John M. Johnston, "Recent developments in the mechanism of the intestinal absorption of fatty acids."

Cell Structure and Metabolism

Richard B. Roberts, *chairman*
Thomas F. Anderson, *vice chairman*

The Cytology of Bacteria and Other Microorganisms

19 June. Structure of microorganisms (R. Williams, *chairman*): G. Chapman, A. Glauert (subjects to be announced). *Structure of microorganism* (continued) (C. F. Robinow, *chairman*): E. S. Anderson, P. C. Fitz-James (subjects to be announced).

20 June. The cell surface (T. F. Anderson, *chairman*): W. Weidel, G. Maccacaro (subjects to be announced). *Radiation analysis of cell structure* (E. C. Pollard, *chairman*): D. Billen, W. R. Guild, F. Hutchinson, J. W. Preiss (subjects to be announced).

21 June. Autoradiographs, suicide (O. Maaløe, *chairman*): F. Forro, M. Riley (subjects to be announced). *Isolation of sub-cellular components* (K. McQuillen, *chairman*): P. A. Albertsson, D. Ezekial (subjects to be announced). *Role of cellular structures in synthesis* (H. Chantrenne, *chairman*): R. J. Britten, J. P. Waller (subjects to be announced). *Transport mechanisms* (A. K. Solomon, *chairman*): A. Képés, S. G. Schultz (subjects to be announced).

23 June. Cell free systems (S. Spiegelman, *chairman*): B. Hall, J. Hurwitz, D. Novelli (subjects to be announced).

Physical Metallurgy

Michael Bever, *chairman*
W. D. Robertson, *vice chairman*

Imperfections, Substructure, and Microstructure

26 June. K. L. Moazed and G. M. Pound, "Field emission microscopy of metal crystal nucleation from the vapor"; C. Elbaum, "Substructures in crystals grown from the melt"; S. Weissmann, "Analysis of substructure—experiments and theory"; C. N. J. Wagner, "Analysis of defect structures in deformed crystals."

27 June. P. B. Hirsch, "Dislocation distributions, flow stress, and hardening in face-centered cubic metals"; U. F. Kocks, "Special arrangements of dislocations in deformed face-centered cubic metals"; V. V. Damiano, "Dislocations and substructures in zinc single crystals"; L. M. Clarebrough, "Short-range order, point defects, and dislocations in alpha brass"; J. R. Patel and A. R. Chaudhuri, "Defect structure in dislocation-free crystals of silicon and germanium."

28 June. M. J. Marcinkowski, "Theory and direct observation of antiphase domain boundaries and dislocations in AB_3 type superlattices"; J. S. Kirkaldy, "The thermodynamic basis of morphological development"; V. A. Phillips, "Electron microscopic observations on the role of dislocations in precipitation"; H. I. Aaronson, "The role of interfacial structure in diffusional transformations."

29 June. M. Hillert, "Some theoretical aspects of the formation of proeutectoid structures"; W. L. Grube and S. R. Rouze, "Growth studies of isothermal transformation structures of austenite by thermionic emission microscopy"; and W. S. Owen, "Structures produced by diffusionless transformations."

30 June. J. E. Hilliard, "Quantitative electron transmission metallography"; M. J. Fraser, "Annealing twins and surface topography"; S. V. Radcliffe, "Products of transformations at high pressure."

Vitamins and Metabolism

Harry P. Broquist, *chairman*
James S. Dinning, *vice chairman*

3 July. (K. Folkers, *chairman*): H. Weisbach, "The cobamide coenzymes"; Y. Kaziro and S. Ochoa, "The role of biotin and vitamin B_{12} in propionic acid metabolism"; (E. Stadtman, *discussant*). (B. C. Johnson, *chairman*): J. C. Rabinowitz, "The role of vitamin B_{12} derivatives in CO_2-pyruvate exchange reactions"; R. H. Abeles, "An intramolecular oxidation-reduction requiring

a cobamide coenzyme"; (L. A. Manson, *discussant*).

4 July. (T. H. Jukes, *chairman*): J. Buchanan, "A new derivative of tetrahydrofolic acid as an intermediate in methionine biosynthesis"; L. Jaenicke, "Tetrahydrofolic acid and the biogenesis of methionine"; (J. C. Rabinowitz, *discussant*). (G. Kidder, *chairman*): G. M. Brown, "The biosynthesis of folic acid" (S. Kirschfeld, *discussant*): G. Plaut, "The biosynthesis of riboflavin and certain pteridines."

5 July. (Chairman to be announced): F. L. Crane, "Benzoquinones in phosphorylating electron transport systems"; C. H. Shunk, A. C. Page, Jr., K. Folkers, "Studies in coenzyme Q, chromenols, and chromanols"; A. F. Brodie, "Napthoquinone coenzymes and a napthochromanol intermediate in oxidative phosphorylation (bacterial)." (E. L. R. Stokstad, chairman): K. Schwarz, "On the mode of action of vitamin E"; S. R. Ames, "The present status of the vitamin A isomers"; J. E. Dowling, "The biological activity of vitamin A acid"; G. Wolf, "Vitamin A function in mucopolysaccharide and adrenal hormone biosynthesis."

6 July. (I. C. Gunsalus, *chairman*): L. O. Krampitz, "Mode of action of thiamine diphosphate"; Q. H. Gibson, "Study of the reaction mechanism of lipoyl dehydrogenase by rapid spectrophotometry"; D. R. Sanadi, "The role of lipoic acid and other disulfides in oxidative reactions." (J. S. Dinning, *chairman*): B. Magasanik, "Control of purine nucleotide biosynthesis and interconversions"; L. Gorini, "Genetic and physiological aspects of the control of arginine biosynthesis."

7 July. (H. P. Sarett, *chairman*): N. Sloane, "Biological activity and chemical properties of metabolites of para-aminobenzoic acid"; J. R. D. McCormick, "Biosynthesis of tetracyclines: cosynthetic factor I"; S. P. Mistry, "Amino acid incorporation and biotin deficiency."

Chemistry, Physiology, and Structure of Bones and Teeth

William P. Norris, *chairman*
Roy V. Talmadge, *vice chairman*

10 July. Selected communications (Robert E. Rowland, *chairman*). *Evolution of bone as a tissue* (Marshall R. Urist, *chairman*): Robert H. Denison, "The habitat of the earliest vertebrates"; Tor Ørvig, "The skeleton of the earliest vertebrates."

11 July. *Evolution of bone as a tissue* (continued) (Marshall R. Urist, *chairman*): Alfred S. Romer, "Vertebrate paleontology"; T. Dale Stewart, "The last phase of human evolution." *Vasculature and hemodynamics of bone* (Robert B. Duthie, *chairman*): J. Trueta, "Anatomical considerations of circulation."

12 July. *Vasculature and hemodynamics of bone* (continued) (Robert B. Duthie, *chairman*): Donald A. McDonald, "Hemodynamics of blood flow"; Friedrich Wassermann, "The problem of transport between blood and bone"; James B. Wray, "The response of periosteal vessels to certain vasomotor agents." *Physical and chemical reactions of bone mineral* (William F. Neuman, *chairman*): John Strandh, "Microchemical studies on single Haversian systems."

13 July. *Physical and chemical reactions of bone mineral* (continued) (William F. Neuman, *chairman*): Eubert J. Daniel, "Histochemical and electron-optical analyses of calcifying systems: part 1, embryogenesis; part 2, maturation"; Howard M. Myers, "Absorption of water and alizarin on apatites"; Aaron S. Posner, "Hydrogen bonding in apatites." *Defects in mineralization and in mineral reserve* (Robert P. Heaney, *chairman*): Robert Steendijk, "Observations on calcification in vivo as affected by intravenous administration of inorganic phosphate"; Leo Lutwak, "Osteoporosis and its treatment, as evaluated by calcium-45."

14 July. *Defects in mineralization and mineral reserve* (continued) (Robert P. Heaney, *chairman*): Jenifer Jowsey, "The structure of bone in relation to age and osteoporosis"; Eugene Eisenberg, "Bone formation and metabolism in relation to osteoporosis and the aging process."

Organic Coatings

G. G. Schurr, *chairman*
R. R. Myers, *vice chairman*

17 July. R. D. Andrews, "Structural analysis of polymers by stress birefringence measurements"; T. L. Smith, "Mechanical properties of polymers."

18 July. E. G. Bell, "Dialysis techniques for study of emulsifier transport phenomena in latex"; E. R. Moore, "Relations between structure and properties in water-thinned alkyd systems."

19 July. J. G. Brodnyan, "Mechanism of emulsion polymerization and particle size distribution functions"; R. F. Fischer, "Cross-linked polyesters from allyl glycidyl ether and maleic anhydride."

20 July. F. W. Hobden, "The mechanism of the formation of bloom on paint surfaces"; J. S. Mackie, "The weathering of paints—an electron microscopic investigation."

21 July. R. J. Fahl, "Optical properties of pigmented films."

Chemistry at Interfaces

R. A. Beebe, *chairman*
L. E. Copeland, *vice chairman*

24 July. *Monolayers on liquids and in biological interfaces* (George L. Gaines, Jr., *chairman*): Norman L. Gershfeld, "The interaction of drugs with model systems"; R. F. Robertson, "Isotopic halogen exchange in unimolecular films at the air-water interface"; Hans J. Trurnit, "Intracellular membranes in relation to photosynthesis."

25 July. *Capillarity* (James C. Melrose, *chairman*): F. P. Buff, "The molecular foundations of the theory of capillarity"; L. E. Scriven, "Dynamics of fluid interfaces"; R. E. Johnson, Jr., "Wettability theory and practice"; B. V. Derjaguin, "The capillary effects of the second kind."

26 July. *Physical adsorption* (Stephen Brunauer, *chairman*): Peter Cannon, "The effects of adsorbed vapors on dislocations and the mechanical properties of solids"; G. L. Kington, "Some thermodynamic aspects of adsorption in porous solids"; J. H. deBoer, "Physical adsorption as a tool for the investigation of the sizes and natures of surfaces."

27 July. *Chemisorption* (Carl H. Amberg, *chairman*): P. A. Redhead, "Chemisorption of diatomic gases on tungsten"; P. J. Lucchesi, "The infrared spectra of acetylene and acetylene derivatives adsorbed on alumina and silica"; B. M. W. Trapnell, "Mechanisms of chemisorption."

(Additional speakers and discussions to be announced.)

Solid-State Studies in Ceramics

L. Himmel, *chairman*
J. Selsing, *vice chairman*

Transport Properties

31 July. F. J. Morin, "The electrical behavior of halides, oxides, and sulfides of the transition metals"; D. H. Whitmore, "The semiconducting properties of transition metal oxides."

1 Aug. H. P. R. Frederickse, "The properties of electrons in rutile"; E. E. Kohnke, "Electrical and optical properties of stannic oxide crystals"; S. P. Mitoff, "Electronic and ionic conductivity in magnesium oxide crystals."

2 Aug. C. J. Kevane, "Electrical properties of rare earth oxides"; S. Kurnick, "Transport processes in rare earth sulfides"; R. Heikes, "Transport phenomena in oxide systems."

3 Aug. J. A. Cape, "High temperature thermal properties of refractory materials"; G. Gody, "Thermal conductivity of semiconducting materials at high temperatures"; W. R. Riedel, "Project Mohole."

4 Aug. H. Schmalzried, "Disorder and diffusion in phases with spinel structure"; R. J. Charles, "Polarization and diffusion in glass."

Toxicology and Safety Evaluations

Frank R. Blood, *chairman*
C. Boyd Shaffer, *vice chairman*

7 Aug. (David W. Fassett, *chairman*): J. M. Coon, "Safety evaluations using humans"; M. L. Keplinger, "The evaluation of toxic materials using humans"; T. B. Clarkson, "The pigeon as an experimental animal"; Martin Ross, "Spontaneous diseases in animals."

8 Aug. (Jack Frawley, *chairman*): R. B. March, "Metabolism of organophosphorus pesticides"; W. J. Hayes, Jr., "Metabolism of chlorinated hydrocarbon insecticides in mammals." (Joseph Treon, *chairman*): M. R. Zavon, "Interpretation of animal toxicity data in human safety evaluation of pesticides"; B. R. Zeitlin, "Use of radioactive glucose for screening of potential toxicants."

9 Aug. (C. Boyd Shaffer, *chairman*): Philippe Shubik, "Evaluation of carcinogenic effect of food additives"; J. A. Miller, "A new metabolic approach in the study of carcinogenic amines." (J. Siegel, *chairman*): W. L. Anderson, "The chemical phase of air pollution in confined spaces"; W. E. McConnaughey, "Engineering aspects of atmosphere control."

10 Aug. (F. H. Snyder, *chairman*): J. H. Draize, "Appraisal of the safety of substances applied topically to skin and mucous membranes"; A. M. Kligman, "Predictive methods for recognizing cutaneous allergens and irritants"; W. Lammers, "Discussion of the margarine additive intoxication." (P. E. Johnson, *chairman*): W. J. Darby, "Food additives as an international problem."

11 Aug. (A. J. Lehman, *chairman*): R. A. Gessert, "Drug evaluation for veterinary use"; H. M. Peck, "Drug interaction and safety evaluation."

Chemistry and Physics of Solids

Walter Kohn, *chairman*
Charles P. Slichter, *vice chairman*
New Theoretical Viewpoints and Their Experimental Significance

14 Aug. Introductory session: W. Kohn and D. Pines. J. M. Luttinger and B. Pippard, "The normal metal."

15 Aug. R. A. Ferrell and J. Friedel, "The normal metal" (continued); J. Bardeen and M. Tinkham, "Superconductivity."

16 Aug. F. Keffer, H. Suhl, B. Matthias, "Magnetism"; R. Kubo and B. Lax, "Transport phenomena."

17 Aug. F. J. Morin and a speaker to be announced, "Insulators"; J. C. Phillips and C. P. Slichter, "New one electron theories."

18 Aug. Concluding discussion: P. W. Anderson and C. Herring.

Photonuclear Reactions

James E. Leiss, *chairman*
Peter T. Demos, *vice chairman*

21–25 Aug. Photon-induced reactions: "Survey of present experiment knowledge of photon-induced reactions"; "Sum rules for photon-induced reactions"; "Theories of photo-nuclear reactions"; "Final states in the nuclear photo effect"; "Inelastic particle scattering"; Theory of coulomb excitation in the medium energy range"; "Electron-induced reactions"; "Nuclear excitation by heavy particle coulomb scattering."

High-Temperature Chemistry: Molten Salts

Dieter Gruen, *chairman*
John Corbett, *vice chairman*

28 Aug. Structural models (J. O'M. Bockris, *chairman*): J. O'M. Bockris, "Models for fused salts"; H. Eyring and D. Ree, "Significant structure theory of fused salts"; A. R. Ubbelohde, "Quasi-crystalline and other models for fused salts"; H. Bloom, "Structure of molten binary mixtures of alkali halides with cadmium halides." *Transport properties* (B. R. Sundheim, *chairman*): B. R. Sundheim, "Transport properties in fused salts"; J. Katz, "Statistical mechanics of transport processes"; F. Collins, "Diffusion in fused salts."

29 Aug. Transport properties (continued) (R. W. Laity, *chairman*): R. W. Laity, "Diffusion in fused salts"; S. Bruckenstein, "Diffusion coefficients and heats of activation for diffusion from chronopotentiometric studies in fused salts"; S. W. Mayer, "Structural studies in polymeric sodium metaphosphate melts." *Thermodynamic properties* (O. J. Kleppa, *chairman*): H. Reiss, "Aspects of the statistical mechanics of fused salts"; H. Flood, "Thermodynamics and structure of fused salts"; O. J. Kleppa, "Reaction calorimetry in fused salt mixtures"; S. Cantor, "Cryoscopy in sodium fluoride."

30 Aug. Electrode processes and electrochemistry (M. Blander, *chairman*): J. O'M. Bockris, "The mechanisms of electrodeposition in fused salts: *a*) Charge transfer to complexes, *b*) Mechanisms of the formation of dendritic deposits"; J. Braunstein, "EMF measurements with silver-solid silver halide electrodes in molten nitrates"; M. Blander, "Interpretation and evaluation of higher association constants from EMF data"; C. H. Liu, "Electrochemistry in molten sulfates and in low melting halides." *Spectroscopy* (M. A. Bredig, *chairman*): F. H. Stillinger, "Theory of radial distribution functions of fused salts"; J. Zarzycki, "Magnetic rotatory polarization"; H. Levy, "X-ray and neutron diffraction studies"; L. S. Singer, "Electron spin resonance."

31 Aug. Spectroscopy (G. P. Smith, *chairman*): G. Janz, "Raman spectra and the structure of fused salts"; G. P. Smith, "Charge transfer spectra in fused salts"; Gordon Goodman, "The possible uses of ligand field theory in studies of fused salts." *Metal-molten salt systems* (J. Corbett, *chairman*): M. Bredig, "Electrical conductivities in a variety of metal-molten halide systems"; J. Corbett, "Metal-metal iodide solutions for the lighter lanthanides"; N. Nachtrieb, "Magnetic susceptibility measurements on the Bi–$BiCl_3$ and the Cd–$CdCl_2$ molten systems"; S. Yosim, "Electrochemical properties of bismuth-bismuth trihalide solutions"; J. W. Tomlinson, "Recent work on metal-metal halide systems."

1 Sept. Advances in technology (R. Steunenberg, *chairman*): W. Grimes, "Molten fluoride systems as fuels and blankets for nuclear reactors"; R. C. Werner, "Fused salts in regenerative fuel cells"; T. A. Henrie and D. H. Baker, Jr., "Some theoretical and practical aspects of fused salt electrolysis of reactive metals."

Tilton School

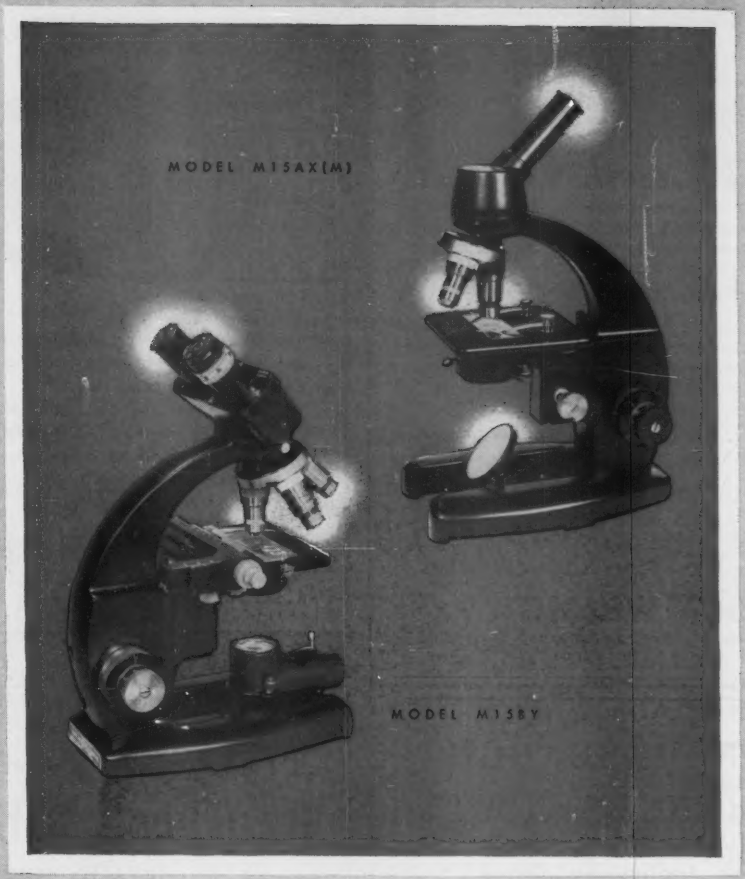
Ion Exchange

Harold F. Walton, *chairman*
Robert M. Wheaton, *vice chairman*

26 June. Complex ions: H. P. Gregor, "Polyelectrolyte-metal complexes"; F. Helfferich, "Ligand exchange"; Y. Marcus, "Anion exchange specificity for metal complexes"; D. W. Margerum, "Kinetics of ion exchange diffusion and coordination reactions."

27 June. Complex ions, thermodynamics: W. Riemann, III, "Kinetics of chelating resins"; C. E. Wymore, "Resins as desiccants"; D. H. Freeman and G. Scatchard, "Swelling of resins studied photomicrographically"; D. Reichenberg, "Exchange distributions with partially sulfonated polystyrenes"; O. D. Bonner, "Model compounds for ion exchange."

28 June. Membranes: R. Schlögl, "Onsager coefficients in membrane transport"; W. Juda, "Applications of ion exchange membranes"; K. W. Chen, "Water desalting by membranes"; Y. Tsunoda and M. Seko, "Water desalting by membranes."



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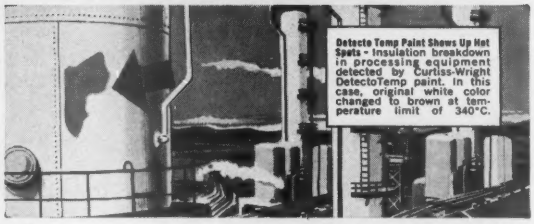

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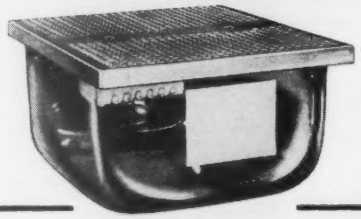

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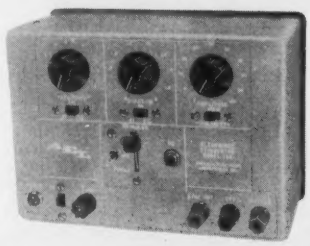

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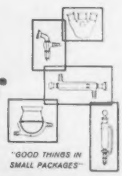

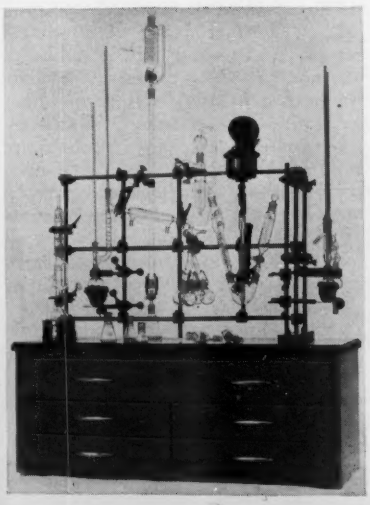

29 June. Special exchangers, column separations: R. Kunin, "Porous resins"; S. Lindenbaum, "Liquid ion exchangers"; W. J. Sloan, "Computers in column problems"; P. B. Hamilton, "Amino-acid separations."

30 June. Panel discussion: *Important unsolved problems in ion exchange* (G. E. Boyd and D. Reichenberg, *chairmen*).

Chemistry of Carbohydrates

3–7 July. (Program to be announced.)

Chemistry and Metallurgy of Semiconductors

A Rosenberg, *chairman*
P. Egli, *vice chairman*

10 July, Epitaxial crystal growth: A. P. Hale, "Vacuum deposition of silicon layers"; T. Renner, "Preparation of III–V compounds by vapor deposition"; V. J. Lyons, "Vapor growth of gallium arsenide"; P. I. Pollak, "Vapor phase growth of silicon crystals."

11 July. Crystal growth at high pressures: W. V. Wright, "Moderately high pressures"; R. H. Wentorf, "Ultra high pressures." *Thermodynamics*: M. B. Bever, "Properties of compound semiconductors"; W. Tiller, "Obtaining phase diagrams through controlled solidification."

12 July. Chemical bonds and electron energy bands: C. H. L. Goodman, A. J. Cornish, "Empirical relations between electrical properties and composition"; J. C. Slater and P. O. Lowdin, "Theoretical relationships."

13 July. Organic conductors: D. Fox, "Mechanisms"; M. I. Pope, "Phenomena."

14 July. Electroluminescence: E. E. Loebner, "Mechanisms"; H. F. Ivey, "Phenomena."

Microbiological Deterioration

James W. Clapp, *chairman*
Arthur M. Kaplan, *vice chairman*

17 July. Newer developments in industrial preservations (Charles C. Yeager, *chairman*): Richard C. Ross, "The microbiology of oil paints"; Robert L. Johnson, "Synthetic latex preservation." *Microorganisms and hydrocarbons* (Ray C. Allred, *chairman*): J. B. Davis, "Oxidation of alkyl-substituted cyclic hydrocarbons"; John O. Harris, "Oxidation of alphaltic hydrocarbons"; Ray C. Allred, "Oxidation of sulfonated alkylbenzenes."

18 July. Mechanisms of action of biocides (Robert E. Deems and Philip N. Gordon, *cochairmen*): Adrien Albert, "The physics and chemistry of fungicidal and bactericidal action"; J. L. Strominger, "Antibiotics as inhibitors of bacterial cell wall synthesis"; M. W. Allen, "Mechanism of action of nematocides"; A. S. Crafts, "Mechanism of ac-

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tion of herbicides"; Saul Rich, "Future trends in the chemical control of microorganisms."

19 July. Spore germination and physiology (Z. John Ordal, *chairman*): Hillel S. Levinson, "Bacterial spores"; S. G. Knight, "Fungal spores." *Genetics and the control of microorganisms* (Harold H. Smith, *chairman*): L. S. Baron, "Genotypic, phenotypic, and environmental effects of genetic transfer mechanisms in microorganisms"; Ellis Engelsberg, "Mutation to sensitivity to carbohydrate inhibition."

20 July. Degradation of cellulose at the enzymic level (R. C. Quittenton, *chairman*): D. R. Whitaker, G. Halliwell (subjects to be announced). *Microbiology of stored grains* (Robert L. Johnson, *chairman*): Clyde M. Christiansen, "Industrious fungi."

21 July. Summary and open discussion of the week's papers and their implications (Adrien Albert, *chairman and discussion leader*).

Biochemistry and Agriculture

James L. Liverman, *cochairman*
Frank L. Stark, Jr., *cochairman*

24 July. The role of cobalt in growth processes (James L. Liverman, *chairman*): Harold Evans, "Cobalt in plant nutrition"; Frank Salisbury, "Cobalt in photomorphogenic responses"; Lars Loercher, "Metabolic aspects of cobalt action"; Horace A. Barker, "Role of vitamin B_{12} in biochemical processes"; Gunther Eichorn, "Chelation in biological phenomena."

25 July. Progress in phenoxy herbicides (John B. Hanson, *chairman*): Corwin Hansch, "Relation of structure to activity" (*A*); Donald G. Crosby, "Relation of structure to activity" (*B*); J. L. Key, "Penetration and translocation"; Michael K. Bach, "Metabolism of the herbicides in plants"; J. B. Hanson, "Effects on the cytoplasm of plants."

26 July. Biochemistry and structure of the plant cell wall (A. C. Neish, *chairman*): S. T. Bayley, "The structure of the plant cell wall and its development"; Peter Albersheim, "The metabolism of pectic substances"; J. R. Colvin, "The mechanism of formation of cellulose fibrils"; M. B. Perry, "Biogenesis of carbohydrates"; H. S. Stafford, "Problems in lignification."

27 July. New aspects of plant disease control (Donald G. Crosby, *chairman*): Arthur Kelman, "Pectinolytic and cellulolytic enzymes in plant disease"; H. H. Flor, "Host-parasite relationships in plant disease"; Ernest Jaworski, "Genetic and antigenic relationships in plant disease." *Role and mission of the President's Science Advisory Boards.*

28 July. New developments in insect control procedures (Frank L. Stark, Jr., *chairman*): Martin Jacobson, "Insect sex attractants in survey and control procedures"; L. D. Christenson, "Promising new approaches to insect control and eradication"; R. C. Von Borstel, "Genetic methods for insect control."

Electrodeposition

Martin S. Frant, *cochairman*
Walter R. Meyer, *cochairman*

31 July. The mechanism of electrodeposition (W. R. Meyer, *chairman*): N. B. Hackerman, "Nature and kinetics of discharge species at the electrode surface"; P. Delahay and D. Mohilner, "Metal deposition: double layer effects and investigation of fast discharge processes." *The mechanism of electrodeposition* (continued) (E. Saubestre, *chairman*): J. O'M. Bockris, "Rate-determining steps and paths in the electrodeposition and electrodissolution of thin metallic layers"; J. O'M. Bockris, D. Drazic, H. Kitz, "Transient phenomena at the iron-solution interface, and the mechanism of the deposition and dissolution of iron."

1 Aug. The mechanism of electrodeposition (continued) (N. Hackerman, *chairman*): R. G. Barradas and B. E. Conway, "Electrochemical adsorption heterocyclic bases and ionic derivatives." Late papers and general discussion on mechanisms. *Addition agents in electrodeposition* (O. Kardos, *chairman*): H. Brown, "The role and structure of addition agents in electroplating, with special reference to nickel"; D. Trivich, "Some aspects of brightener ac-

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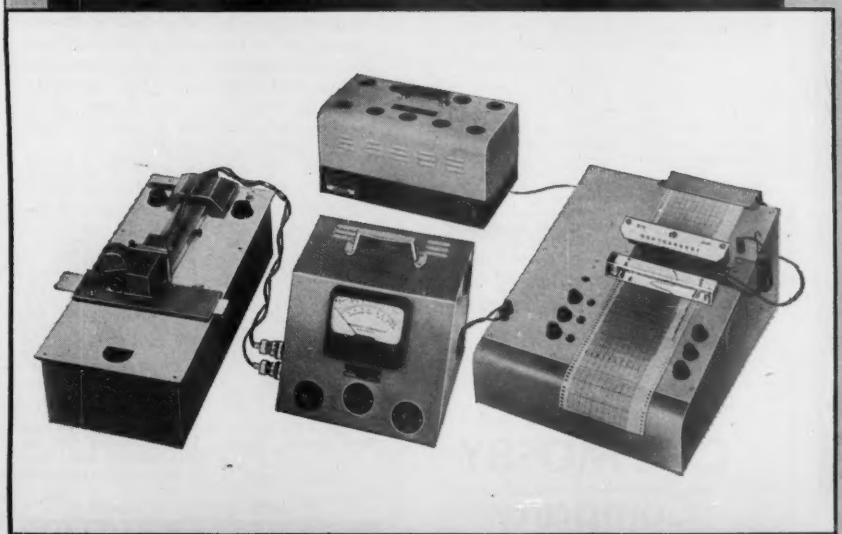

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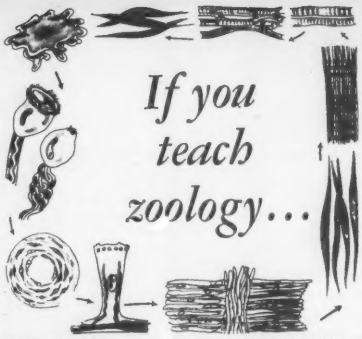

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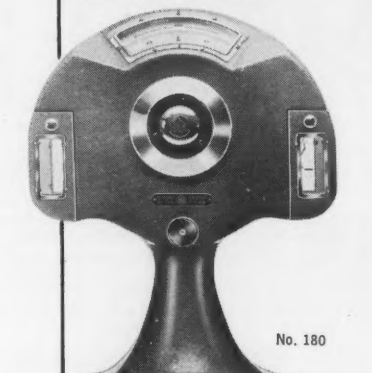

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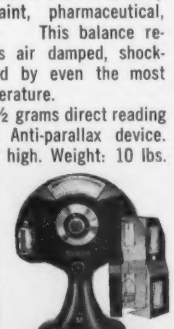

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tion"; S. E. Beacom, "Leveling in bright nickel deposits."

2 Aug. The effect of structure on properties: *A) Basis metal structure* (A. Brenner, *chairman*): H. Leidheiser, "Nickelplating on copper single crystals"; M. H. Jones, "Effect of substrate metallurgy." *B) Coating metal structure* (A. Brenner, *chairman*): H. J. Read, "Plastic properties of electrodeposits"; A. M. Max, "Stress in electrodeposits."

3 Aug. Electrodeposition of alloys (F. A. Lowenheim, *chairman*): M. L. Holt, "Electrodeposition of alloys of some of the transition metals"; H. Koretzky, "Current research on electrodeposited thin magnetic films. A critical survey"; I. Wolf, "Factors affecting magnetic properties of iron-nickel films." *Electrodeposition on unusual substrates* (H. B. Linford, *chairman*): D. R. Turner, "Metal deposition on semiconductors"; E. Saubestre, "Plating on unusual metals."

4 Aug. Current problems in the utilization of plated coatings (N. Murphy, *chairman*): M. Frant, "Electrodeposited metals as electrical contacts"; F. A. Lowenheim, "Solderability of plated metals"; C. Levy, "Electro-deposited coatings for high temperature applications."

Electrical and Relaxation Processes in Glass

R. J. Charles, *chairman*
N. J. Kreidl, *vice chairman*

7 Aug. Electrical conduction and polarization (R. J. Charles, *chairman*): H. E. Taylor, "The dielectric relaxation of glass due to the movement of alkali ions"; J. I. Isard, "Electrical conduction in glasses." (N. J. Kreidl, *chairman*): A. E. Owen, "Comparison of conduction and relaxation processes in glass."

8 Aug. P. M. Sutton, "Space charge and electrode polarization"; R. J. Charles, "A defect model of diffusion in glass." *Alkali ion mobility* (F. M. Ernsberger, *chairman*): T. Abe, "On the free sodium ion in glass"; G. Eisenman, "The electrochemical properties of cation-sensitive glass electrodes and the atomic basis of their cation discrimination."

9 Aug. R. J. Ryder, "Internal friction of alkali silicate glasses"; I. I. Kitaigorodskii, (subject to be announced). *Glass structure and the liquid state* (A. B. Bestul, *chairman*): D. Turnbull, "The glass transition."

10 Aug. P. D. Bray, "Nuclear magnetic resonance studies of the structure of glass"; M. Aslanova, "Structure and properties of glass fibers"; T. A. Litovitz, "Structural relaxation in liquids (molten glasses)."

11 Aug. Discussions and business meeting.

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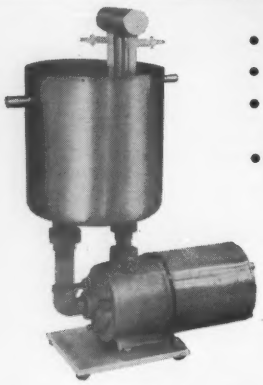

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Humid Tropics Vegetation

As a contribution toward the UNESCO Humid Tropics Programme, a group of scientists representing a number of fields came together from 14 to 20 September 1960, at Goroka, Territory of New Guinea, for a symposium on "The Impact of Man on the Vegetation of the Humid Tropics." Most of the participants were from southeast Asia and Australia and intermediate areas, but one, E. J. H. Corner, was from England and two, J. M. Blaut and F. R. Fosberg, were from the United States. The UNESCO Southeast Asia Science Cooperation Office and the Administration of the Territory of New Guinea cooperated in the enterprise, and the principal credit for superb local arrangements goes to John Womersley, of the Division of Botany, Lae, New Guinea.

The attention of the symposium was largely directed to the effects of the activities of man prior to the era of the chain saw and bulldozer. Goroka, in the middle of a vast area changed in prehistoric time from rain forest to grassland and cultivation by people who did not even have metal implements, was a truly appropriate place for discussion of such a subject. One had only to look out the window to see illustrations of what was being discussed. Present were literate representatives of peoples who, in 1930, had not yet seen a white man. Many of the participants and observers were members of the Administration of the Trust Territory of New Guinea, who deal daily with the matters under discussion. These factors gave a sense of reality that such conferences seldom have.

After a formal opening by J. T. Gunther, Assistant Administrator of the Territory, and discussion of the physical and human background, the subject was handled under seven broad headings, much more attention being given to the anthropological, social, and economic aspects than is indicated by the symposium title. (i) Consideration of the effect of selection and cultivation of food plants brought out that the exploitation of wild plants leads imperceptibly to cultivation, that wild species are changed by selection resulting in cultigens which may replace their wild ancestors, that the principal effect of food gathering on the forest is to increase the proportion of trees with edible fruits, and that gardening has far greater effect than this on the vegetation. (ii) The use of fire by early man, whether to get rid of debris, to destroy the forest because of its "nuisance value," to aid in hunting, or merely for fun, has been one of the strongest influences on vegetation and has played a large part in converting vast areas in the tropics from rain forest to grass and

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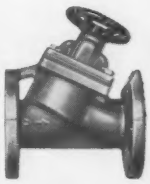

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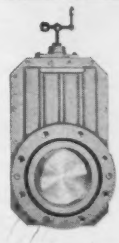

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agricultural land. (iii) Because grazing of domestic animals is not important in New Guinea, this topic received less attention than had been expected, but it was emphasized that in tropical grasslands intensive grazing is necessary, in the absence of fire, to prevent these man-made grass communities from reverting to woody vegetation. (iv) Shifting agriculture, admittedly of major importance in the alteration of tropical vegetation, came in for perhaps a disproportionate share of attention. It was asserted that an equilibrium between man and his environment is possible under this system of agriculture, but that population pressure leads to unbalance. One of the most interesting items brought out was the fact that in the highlands of New Guinea *Casuarina* has long been planted to increase soil fertility, even though its nitrogen-fixing properties were only recently determined scientifically. (v) It was pointed out that secondary plant communities resulting from man's activity are usually less mesophytic than the primary communities that they replace, that this often assists in the detection of such communities in an otherwise natural landscape, and that the reflection of the environmental pattern by vegetation becomes generally more obscured as a result of man's activities. (vi) A general review of the New Guinea grasslands showed that, of the two principal types, tall grass is a successional stage leading directly back to forest, while short grass results from much more profound alteration of the environment through man's activities. (vii) Public-health aspects of the alteration of the vegetation by man were noted, especially changed distribution patterns of disease vector populations.

As a result of the discussions a number of resolutions were formulated and sent to the UNESCO authorities. In essence, these called for the promotion, by UNESCO of (i) research into all aspects of secondary communities produced by human disturbance of the tropical forest; (ii) ethnobotanical research and cooperation in field work between anthropologists and botanists; (iii) investigations into traditional animal husbandry of preliterate societies, and into the psychosociological consequences of introduced pastoralism in primitive horticultural communities in the humid tropics, especially in Oceania; (iv) investigations into the susceptibility of humid tropical limestone soils and vegetation to damage by fire and other agents; (v) investigations into the ecological implications of root-physiology; and (vi) research into the socioeconomic adaptations to alien influence of recently contacted groups of people.

UNESCO was also asked to stimulate effective programs for conserving natural resources in the humid tropics, especially by creating nature reserves and national parks and by obtaining, through education and mass communications media, support of local populations for these measures. It was recommended that UNESCO convene, in two years' time, a symposium on the results of recent ecological research in the humid tropics.

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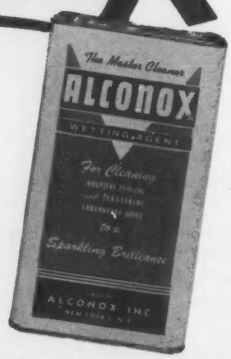

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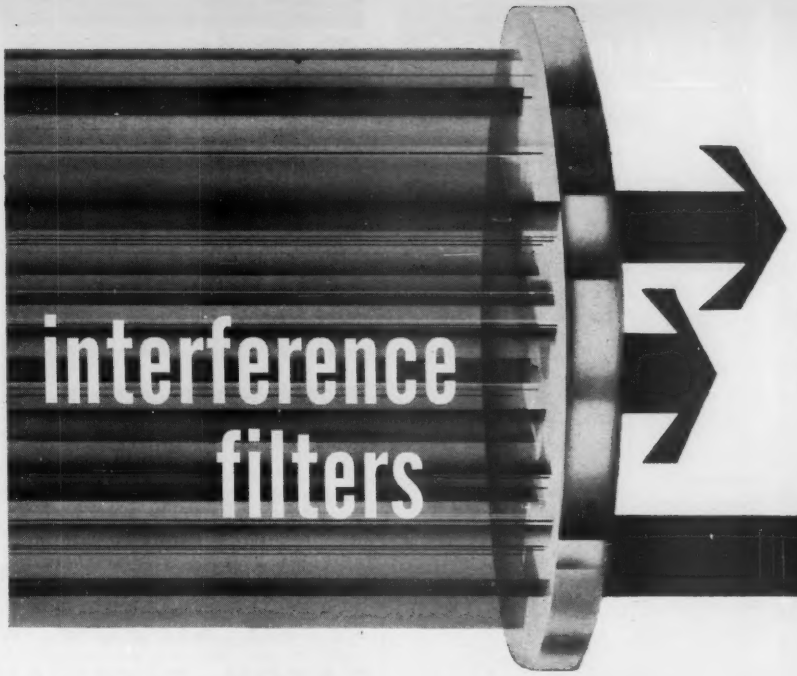
17–18. Great Lakes Research, 4th conf., Ann Arbor, Mich. (C. F. Powers, Great Lakes Research Division, 1119 Natural Science Bldg., Ann Arbor)

17–19. Fluid Seal Meeting, intern., Ashford, Kent, England. (Information Officer, British Hydromechanics Research Assoc., South Road, Temple Fields, Harlow, Essex)

17–24. International Congress of Nurses, 12th quadrennial cong., Melbourne, Australia. (Miss D. C. Bridges, Secretary, 1 Dean Trench St., London, S.W.1, England)

18–20. Chemical Reactions in the Lower and Upper Atmosphere, intern. symp., San



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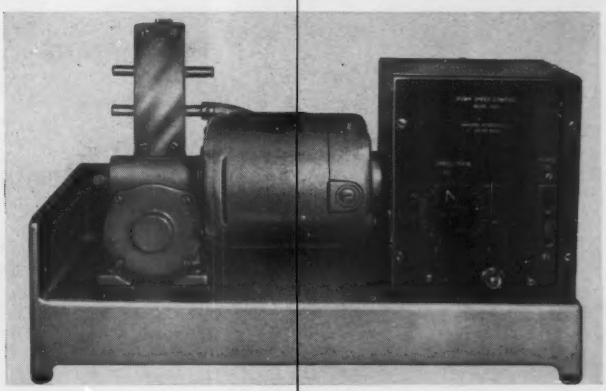

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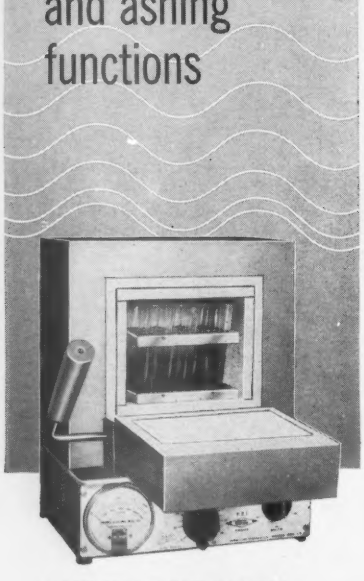

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18–21. American Geophysical Union and American Meteorological Soc., Washington, D.C. (American Geophysical Union, 1515 Massachusetts Ave., NW, Washington 5, D.C.)

19–21. Southwestern Inst. of Radio Engineers Conf. and Electronics Show, Dallas, Tex. (SWIRECO 61, P.O. Box 7443, Dallas 9)

20–21. Society of Chemical Industry, fungicide symp., London, England. (B. J. Heywood, 103 Harrow Drive, Hornchurch, Essex, England)

20–22. Association of Southeastern Biologists, Lexington, Ky. (H. J. Humm, Department of Botany, Duke Univ., Durham, N.C.)

20–24. Microbial Reactions in Marine Environments, intern. symp., Chicago, Ill. (C. H. Oppenheimer, Inst. of Marine Science, Univ. of Texas, Port Arkansas)

21–22. American Assoc. of Univ. Professors, Boston, Mass. (W. P. Fidler, AAUP, 1785 Massachusetts Ave., NW, Washington 6, D.C.)

21–23. American Soc. for the Study of Sterility, annual, Miami Beach, Fla. (H. H. Thomas, 920 S. 19 St., Birmingham 5, Ala.)

23. American Pharmaceutical Assoc., Chicago, Ill. (W. S. Apple, 2215 Constitution Ave., NW, Washington, D.C.)

23–26. American Assoc. of Colleges of Pharmacy, Chicago, Ill. (C. W. Bliven, George Washington Univ., Washington 6, D.C.)

23–27. American Ceramic Soc., 63rd annual, Toronto, Canada. (C. S. Pearce, 4055 N. High St., Columbus 14, Ohio)

23–27. Society of American Bacteriologists, Chicago, Ill. (E. M. Foster, 311 Bacteriology, Univ. of Wisconsin, Madison)

23–28. American Soc. of Hospital Pharmacists, Chicago, Ill. (J. A. Oddis, 2215 Constitution Ave., NW, Washington 7, D.C.)

24–26. Aerospace Medical Assoc., 32nd annual, Chicago, Ill. (W. J. Kennard, Secretary-Treasurer, c/o Washington National Airport, Washington, D.C.)

24–26. American Psychoanalytic Assoc., annual, Philadelphia, Pa. (J. N. McVeigh, 36 W. 44 St., New York 36)

24–27. American Assoc. of Petroleum Geologists, Denver, Colo. (G. V. Cohee, U.S. Geological Survey, Washington 25, D.C.)

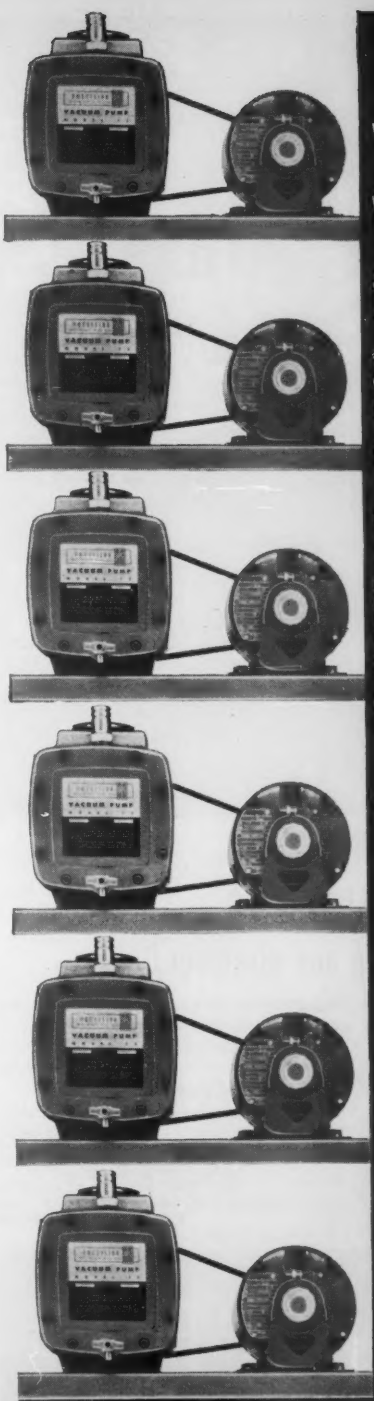
24–27. American Physical Soc., Washington, D.C. (K. K. Darrow, 538 W. 120 St., New York 27)

25–28. Society of Economic Paleontologists and Mineralogists, Denver, Colo. (J. Imbrie, Dept. of Geology, Columbia Univ., New York, N.Y.)

27–28. Diseases in Nature Transmissible to Man, 11th annual southwestern conf., College Station, Tex. (F. P. Jaggi, Jr., Agricultural and Mechanical College of Texas, College Station)

27–28. Health Education Conf., New York Acad. of Medicine, New York, N.Y. (I. Goldston, 2 E. 103 St., New York 29)

27–29. American Acad. of Neurology (members and guests), Detroit, Mich. (Mrs. J. C. McKinley, 4307 E. 50 St., Minneapolis 17, Minn.)



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27–5. American Psychiatric Assoc., annual, Philadelphia, Pa. (D. Blain, 1700 18 St., NW, Washington 6)

28–30. American Psychosomatic Soc., 18th annual, Atlantic City, N.J. (M. F. Reiser, 265 Nassau Road, Roosevelt, N.Y.)

30–4. Aero/Space Instrumentation Symp., 7th annual, Dallas, Tex. (W. J. Gabriel, Route 3, Box 36, Fort Worth, Tex.)

30–4. Electrochemical Soc., Indianapolis, Ind. (R. K. Shannon, 1860 Broadway, New York 23)

30–6. Conference on Internal Medicine, Nassau, Bahamas. (Bahamas Conferences, P.O. Box 1454, Nassau)

May

1–3. American Oil Chemists' Soc., St. Louis, Mo. (K. F. Mattil, Swift and Co., U.S. Yards, Chicago 9, Ill.)

2–3. American Pediatric Soc., Atlantic City, N.J. (C. M. Riley, Denver General Hospital, Denver 4, Colo.)

2–3. Association of American Physicians, Atlantic City, N.J. (P. B. Beeson, Yale Univ. School of Medicine, New Haven 11, Conn.)

2–5. Criticality Control in Chemical and Metallurgical Plant, intern. symp., OEEC, Karlsruhe, Germany. (European Nuclear Energy Agency, 38, Boulevard Suchet, Paris 16, France)

2–6. American Assoc. on Mental Deficiency, Cincinnati, Ohio. (N. A. Dayton, Mansfield Training School, Mansfield Depot, Conn.)

3–5. Nuclear Applications in Space Conf., Gatlinburg, Tenn. (J. J. Harford, American Rocket Soc., 500 Fifth Ave., New York, N.Y.)

3–6. American Goiter Assoc., Philadelphia, Pa. (J. C. McClintock, 702 Madison Ave., Albany 8, N.Y.)

3–6. Midwestern Psychological Assoc., Chicago, Ill. (I. E. Farber, Dept. of Psychology, State Univ. of Iowa, Iowa City, Iowa)

3–7. Student American Medical Assoc., Chicago, Ill. (R. F. Staudacher, 430 N. Michigan Ave., Chicago 11)

4–5. Human Factors in Electronics, 2nd natl. symp., Arlington, Va. (H. P. Birmingham, Human Engineering Development Section, U.S. Naval Research Laboratory, Washington 25)

4–5. Society for Pediatric Research, Atlantic City, N.J. (C. D. West, Children's Hospital, Cincinnati 29, Ohio)

4–6. American Ethnological Soc., Columbus, Ohio. (Miss N. F. S. Woodbury, Arizona State Museum, Univ. of Arizona, Tucson)

4–6. American Philosophical Assoc., western division, St. Louis, Mo. (L. E. Hahn, Washington Univ., St. Louis 30, Mo.)

4–6. American Soc. of Human Genetics, Atlantic City, N.J. (W. J. Schull, 1133 E. Catherine St., Ann Arbor, Mich.)

4–6. Pediatric Surgery, symp., New York, N.Y. (Office of the Associate Dean, New York Univ. Post-Graduate Medical School, 550 First Ave., New York 16)

4–6. Society for American Archaeology, Columbus, Ohio. (J. B. Wheat, Univ. of Colorado Museum, Boulder)

5–6. Population Assoc. of America, New York, N.Y. (K. B. Mayer, Dept. of Sociology and Anthropology, Brown Univ., Providence 12, R.I.)

5–7. American Soc. of Internal Medicine, Miami Beach, Fla. (G. T. Bates, 350 Post St., San Francisco 8, Calif.)

5–8. American Psychoanalytic Assoc., Chicago, Ill. (Mrs. H. Fischer. 1 E. 57 St., New York 22)

6–7. Academy of Psychoanalysis, annual, Chicago, Ill. (J. H. Merin, 49 E. 78 St., New York 21)

6–9. Circuit Theory, 5th midwestern symp., Urbana, Ill. (M. E. Van Valkenburg, Dept. of Electrical Engineering, Univ. of Illinois, Urbana)

7–10. American Inst. of Chemical Engineers, Cleveland, Ohio. (J. F. Van Antwerpen, ALChE, 25 W. 45 St., New York 36)

7–11. Institute of Food Technologists, New York, N.Y. (C. S. Lawrence, 176 W. Adams St., Chicago 3, Ill.)

7–12. Medical Library Assoc., Inc., Seattle, Wash. (Miss R. J. Mann, Mayo Clinic Library, Rochester, Minn.)

7–12. Society of American Bacteriologists, 62nd annual, Kansas City, Mo. (E. M. Foster, 311 Bacteriology, Univ. of Wisconsin, Madison 6)

7–12. Society of Motion Picture and Television Engineers, Toronto, Canada. (SMPTE, 55 W. 42 St., New York 36)

8–9. Titrimetric Methods of Analysis, symp., Cornwall, Ontario, Canada. [J. R. McCallum, Courtaulds (Canada) Ltd., Cornwall]

8–10. Aerospace Electronics Conf., 13th annual natl., Dayton, Ohio. (R. G. Stimmel, Institute of Radio Engineers, 1 E. 79 St., New York 21)

8–10. Instrument Soc. of America, Power Instrumentation Symp., 4th natl., Chicago, Ill. (H. A. Van Wassen, Duquesne Light Co., Pittsburgh 19, Pa.)

8–12. American College of Physicians, 42nd annual, Miami Beach, Fla. (ACP, 4200 Pine St., Philadelphia 4, Pa.)

8–12. American Psychiatric Assoc., 117th annual, Chicago, Ill. (C. H. H. Branch, 156 Westminster Ave., Salt Lake City, Utah)

9–11. Western Joint Computer Conf., Los Angeles, Calif. (W. F. Bauer, 8433 Fallbrook Ave., Canoga Park, Calif.)

10–12. Production Engineering Conf., Toronto, Canada. (R. B. Larson, 5701 Carnegie Ave., Cleveland 3, Ohio)

10–13. National Science Fair—International, 12th, Kansas City, Mo. (Science Service, 1719 N Street, NW, Washington 6, D.C.)

11–13. Acoustical Soc. of America, Philadelphia, Pa. (W. Waterfall, 335 E. 45 St., New York 17)

11–13. American Inst. of Industrial Engineers, annual, Detroit, Mich. (W. J. Jaffe, Newark College of Engineering, 367 High St., Newark 2, N.J.)

11–13. American Radium Soc., Colorado Springs, Colo. (C. G. Stetson, 350 Engle St., Englewood, N.J.)

15–16. Co-ordination Compounds, symp., Hamilton, Ontario, Canada. (R. J. Gillespie, McMaster Univ., Hamilton)

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15–17. Institute of Radio Engineers, natl. symp., Washington, D.C. (G. Shapiro, National Bureau of Standards, Washington 25)

15–17. Radiation Research Soc., annual, Washington, D.C. (E. L. Powers, Div. of Biological and Medical Research, Argonne National Laboratory, Argonne, Ill.)

15–18. Society of Aeronautical Weight Engineers, Akron, Ohio. (D. B. Block, 4004 Oxford Ave., NW, Masillon, Ohio)

15–18. Spectroscopy, 12th annual symp., Chicago, Ill. (W. Ashby, Continental Can Co., Inc., 7622 S. Racine Ave., Chicago 20, Ill.)

15–20. Conference on Nuclear Electronics, Belgrade, Yugoslavia. (J. Burt, International Atomic Energy Agency, United Nations, New York, N.Y.)

16–18. Western Conf. on Anesthesiology, biennial, Portland, Ore. (J. O. Branford, 2307 NW Overton St., Portland 9, Ore.)

16–20. American College of Cardiology, New York, N.Y. (P. Reichert, 350 Fifth Ave., Empire State Bldg., New York 1)

18–20. Host Tumor Interactions, intern. symp., Detroit, Mich. (M. J. Brennan, Oncology Div., Henry Ford Hospital, Detroit 2)

22–24. American Thoracic Soc., Cincinnati, Ohio. (F. W. Webster, 1790 Broadway, New York 19)

22–24. Global Communications, 5th natl. symp., Chicago, Ill. (R. D. Slayton, 5555 Touhy Ave., Skokie, Ill.)

22–24. Telemetering Conf., natl., Chicago, Ill. (J. Becker, AC Spark Plug Division, General Motors Corp., Milwaukee 1, Wis.)

22–25. American Urological Assoc., Los Angeles, Calif. (W. P. Didusch, 1120 N. Charles St., Baltimore 1, Md.)

22–25. Design Engineering Conf. and Show, Detroit, Mich. (ASME Meetings Dept., 29 W. 39 St., New York 18)

22–25. National Tuberculosis Assoc., Cincinnati, Ohio. (J. G. Stone, 1790 Broadway, New York 19)

22–26. Engineering Conf. and Exhibit, 29th annual, New York, N.Y. (G. E. Seeley, ASTME Headquarters, 10700 Puritan Ave., Detroit 38, Mich.)

22–26. Society of Photographic Scientists and Engineers, annual, Binghamton, N.Y. (M. G. Anderson, Ansco, Vestal Parkway East, Binghamton, N.Y.)

22–27. International Acad. of Legal Medicine and of Social Medicine, 5th cong., Vienna, Austria. (M. Helpern, Chief Medical Examiner, City of New York, 55 East End Ave., New York 28)

23–25. Large Capacity Memory Techniques for Computing Systems, symp., Washington, D.C. (Miss J. Leno, Code 430A, Office of Naval Research, Washington 25)

25. Gastroenterology Research Group, Chicago, Ill. (N. C. Hightower, Scott and White Clinic, Temple, Tex.)

25–26. Medical Technology, symp., Cleveland, Ohio. (J. W. King, Cleveland Clinic, 2020 E. 93 St., Cleveland 6)

25–26. Nitro Aliphatic Chemistry, symp., Lafayette, Ind. (Purdue Memorial Union, Purdue Univ., Lafayette)

25–26. Operations Research Soc. of America, 9th annual, Chicago, Ill. (D. Schiller, Gaywood-Schiller Associates, 203 N. Wabash Ave., Chicago 1)

26–27. American Otological Soc., Lake Placid Club, Essex County, N.Y. (J. A. Moore, 525 E. 68 St., New York 21)

26–3. American Acad. of Dental Medicine, cruise to Bermuda and Nassau. (H. Ward, 15 Bond St., Great Neck, N.Y.)

28–1. Special Libraries Assoc., 52nd annual, San Francisco, Calif. (B. M. Woods, SLA, 31 E. 10 St., New York 3)

29–31. American Gynecological Soc., Colorado Springs, Colo. (A. H. Aldridge, 899 Park Ave., New York 21)

29–31. Cancer Symp., 6th annual, Regina, Saskatchewan, Canada. (A. J. S. Bryant, Allan Blair Memorial Clinic, Regina)

29–31. Chemical Inst. of Canada, 44th annual, Ottawa. (Chemical Inst. of Canada, 48 Rideau St., Ottawa 2)

29–3. Molecular Spectroscopy, 5th European cong., Amsterdam, Netherlands. (D. H. Zijp, Secy., Nieuwe Achtergracht 123, Amsterdam-C.)

30. Nutrition Soc. of Canada, 4th annual, Guelph, Ontario. (E. V. Evans, Dept. of Nutrition, Ontario Agricultural College, Guelph)

31–2. Canadian Federation of Biological Societies, Guelph, Ontario, Canada. (E. H. Bensley, Montreal General Hospital, 1650 Cedar Ave., Montreal 25, P.Q.)

31–2. Radar symp., 7th annual, Ann Arbor, Mich. (Coordinator, 7th Annual Radar Symposium, Institute of Science and Technology, Box 618, Ann Arbor)

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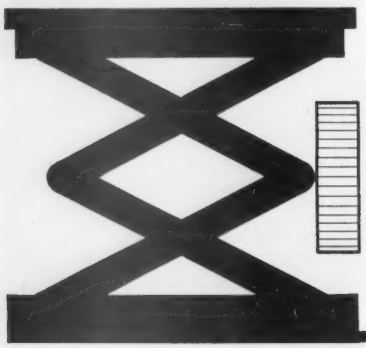

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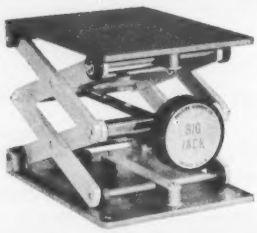

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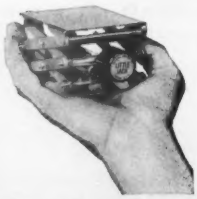

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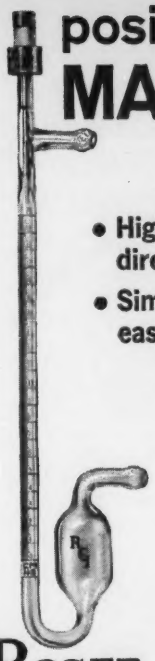

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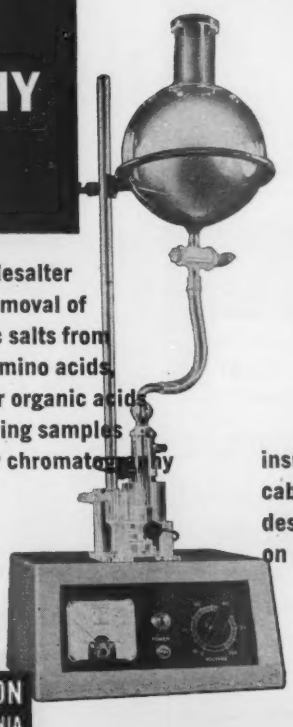
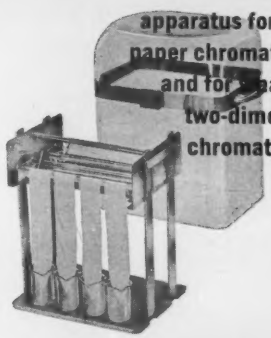
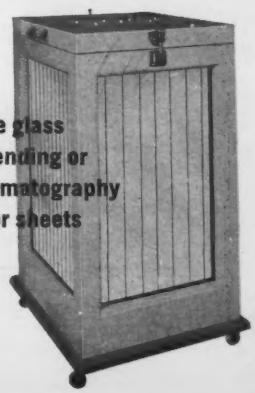


electric desalter for the removal of inorganic salts from sugars, amino acids, and other organic acids in preparing samples for paper chromatography



apparatus for eluting paper chromatograms and for small scale two-dimensional chromatography



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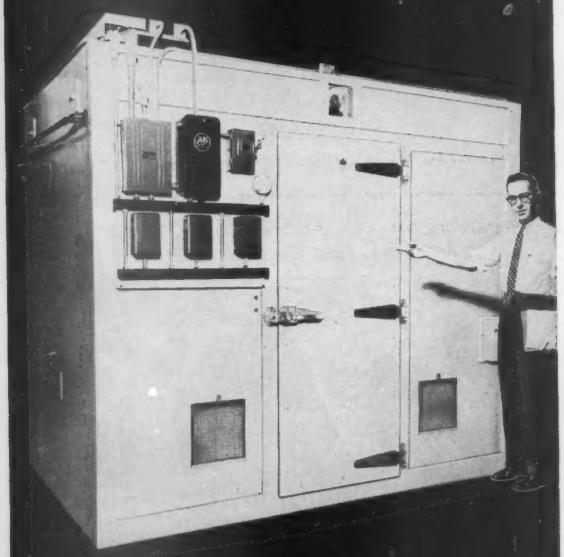

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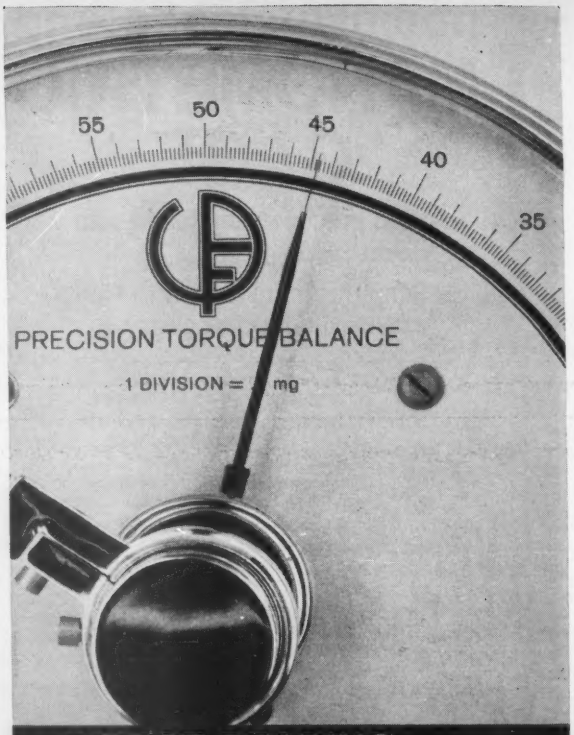

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■ MICROFILM FACSIMILE SYSTEM is capable of transmitting information at rates equivalent to a single-space typing rate of 43,200 words per minute in one model and 20,400 per minute in a second model. Input accepted by the transmitter is a positive or negative microfilm image. The information to be transmitted is scanned and dissected by a mechano-optical device which uses a fixed slot and a multiple-radial-slit rotating disc. The output signal is baseband video plus blacker-than-black synchronizing pulse. Bandwidth is approximately 400 kcy/sec in the faster model. Signal level is 1.0-volt peak; output impedance is 50 ohms. The receiving recorder is a multiple-stylus and printer type with 1800 styluses spaced 0.01 in. apart in the faster model. The record is produced on a special facsimile paper. In the faster model chart length is 1000 ft, chart width is 18.5 in., and chart feed rate is 4 in./sec. Signal input requirements are matched to the transmitter output. (Hogan Faximile Corporation, Dept. Sci123, 635 Greenwich St., New York, N.Y.)

■ ANGLE INDICATOR provides numerical indication of the angular position of mechanical devices to which remote two-speed (25:1) dual transmitters can be coupled. Accuracy of the two-speed unit is said to be ±15 sec of arc, and repeatability is said to be ±12 sec. Slewing speed is 180 deg in 9 sec; operating power is 115 v 400 cy/sec. (General Precision Inc., Dept. Sci115, 1150 McBride Ave., Little Falls, N.J.)

■ ANGLE ENCODING TRANSDUCER AND ROTATIONAL ANALYZER are combined into a system for study of rotation-associated phenomena in machinery (Fig. 1). The two units adapt an oscilloscope to provide horizontal trace deflection proportional to angular displacement of a rotating shaft. The system generates a horizontal sweep representing shaft angle at speeds from essentially 0 to 20,000 rev/min. Transduced data, such as velocity, pressure, acceleration, or vibration, appear on the cathode-ray-tube screen correctly referenced to instantaneous angular position. (Tektronix, Inc., Dept. Sci106, Post Office Box 500, Beaverton, Ore.)

■ LABORATORY PROCESS MONITOR is designed to program, monitor, and control reactions and distillations automatically. Variations in circuitry can be provided to fit individual program needs. Above ambient temperature reactions may be controlled with heaters; below ambient temperature they may be controlled with coolant solenoids activated as required. (Scientific Glass Apparatus Co., Dept. Sci124, Bloomfield, N.J.)

■ TRANSMITTING FLOWMETER combines the functions of an indicating rotameter and a pneumatic transmitter. The instrument measures liquid or gas flow with accuracy said to be within ±1 percent over its 10 to 1 range. The float in the metering tube is magnetically coupled to a rotating follower in the pneumatic transmitter. The transmitter can be used with any metering tube ½ to 2 in. in diameter; it covers flows equivalent to 1 to 100 gal of water per minute and 4 to 400 standard ft^3 of air per minute. Steam-jacketed and high-pressure models are available. (Fischer & Porter Co., Dept. Sci119, 676 Jacksonville Rd., Warminster, Pa.)

■ DISSOLVED OXYGEN ANALYZER measures the concentration of dissolved oxygen in a variety of fluid systems—including sewage; biological, natural, and fresh sea water; and some organic media. Operation is based on the measurement of current produced by the reduction of oxygen at a platinum electrode. The instrument also provides temperature measurements and temperature compensation. The instrument's transistorized amplifier is battery operated and is said to have no zero drift. (Jarrell-Ash Co., Dept. Sci103, 26 Farwell St., Newtonville 60, Mass.)

■ MAGNETIC DRUM UNIT designed for use with the Univac solid-state and STEP computers consists of two drums capable of containing 24 × 10^6 digits and signs. Data are fed directly from the memory to the central computer and returned without intermediate steps

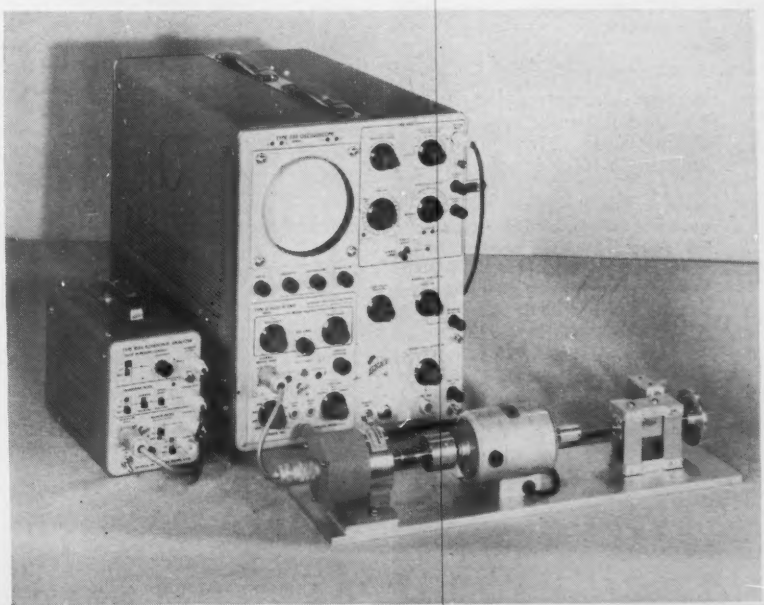

Fig. 1. Angle encoding transducer (right) and rotational analyzer (left).

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. A Readers' Service card for use in mailing inquiries concerning the items listed is included on pages 815 and 929. Circle the department number of the items in which you are interested on this card.*

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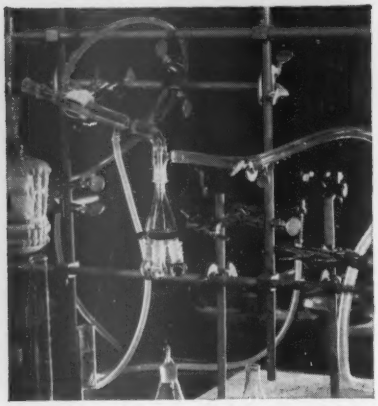

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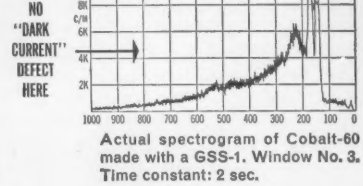
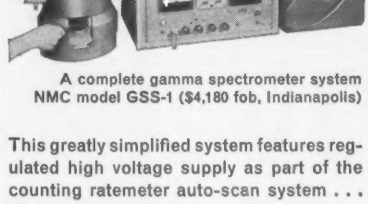


Actual spectrogram of Cobalt-60 made with a GSS-1. Window No. 3. Time constant: 2 sec.



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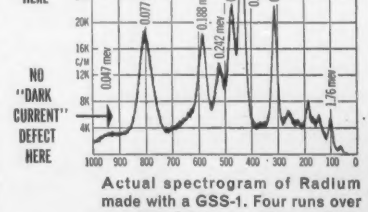

Actual spectrogram of Radium made with a GSS-1. Four runs over a period of 20 hours with no adjustment of instrument between runs, demonstrating utmost stability. Range: 30,000 C/M. Time constant: 3 sec.

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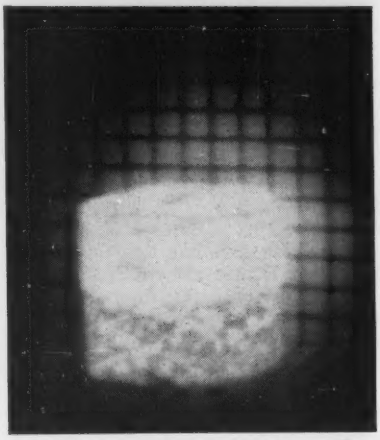

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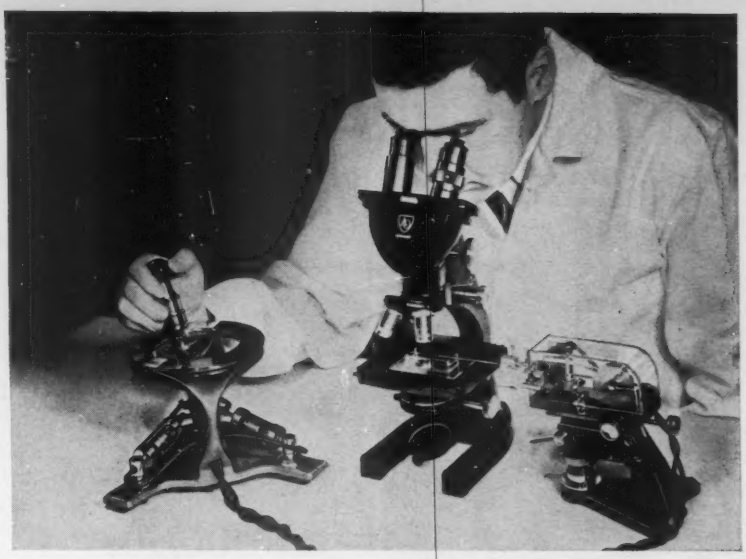
and with automatic check of accuracy. Random access time is on the order of milliseconds. The system can be expanded to as many as 10 storage drums. (Remington Rand, Dept. Sci-117, 315 Park Ave. South, New York 10, N.Y.)

■ LOW LEVEL COUNTING SYSTEM for beta radiation features background count less than 0.25 per minute for a 1-in. diameter detector. Shield size is 18 by 18 in.; shield weight is 1500 lb. The system can be converted to simultaneous alpha-beta measurements on a single sample; fully automatic operation may be added. Automatic readout may be provided in the form of printing timers or a complete preset-count-preset-time readout system with overriding control. Standard detector sizes are ½, 1, 2, and 3 in. (Sharp Laboratories, Inc., Dept. Sci118, P.O. Box 1302, La Jolla, Calif.)

■ VIBRATION CALIBRATOR consists of a transistorized electromechanical oscillator and a battery-operated cylindrical shaker. The instrument provides a standard acceleration for 1 grav r.m.s. at 100 cy/sec. Acceleration output appears at two pillbox-shaped, 50-g disks mounted on an internal cylinder extending through the instrument and projecting at the sides. Acceleration accuracy is said to be ±10 percent, frequency accuracy ±1 percent. Battery life is 100 hr. (General Radio Co., Dept. Sci120, West Concord, Mass.)

■ SWEEP DRIVE automatically sweeps oscillators and other tunable devices through their frequency ranges. The device has two sweep speeds and a neutral setting; in neutral position, tuning can be performed manually. Sweeps can be reversed or stopped automatically when limits are reached. Stops can be set for any sweep range from 5 deg rotation to 50 revolutions of the output shaft. One model provides an output voltage proportional to shaft position for driving the x axis of a recorder or oscilloscope. (Hewlett Packard Co., Dept. Sci125, 1501 Page Mill Rd., Palo Alto, Calif.)

■ HIGH VOLTAGE IMPULSE GENERATORS are available for 700, 1400, 2100, or 3500 kv for use in testing cables, insulating materials, electrical equipment, and lightning protection systems. The equipment utilizes a number of capacitors that are charged up to a maximum of 175 kv when connected in parallel. By means of spark gaps the capacitors are suddenly connected in series, and the test object receives a high voltage impulse which is approximately equal to the product of the number of capacitors and the charging voltage. Spark-gap distance can be adjusted for any



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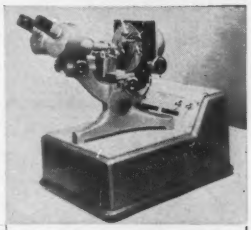

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Manufactured under exclusive license o the French patents, the instrument consists of a manipulator and receiver. Use with any microscope . . . arranged for right or left hand operation. The micro-tool is mounted on the receiver; impulses from the manipulator are transmitted to the tool through sensitive pneumatic membranes. Within a range of 3 mm, the micro-instrument may be moved in any plane or angle by a single control. Ratio of displacement of control lever and micro-tool may be adjusted from 1:50 to 1:2500.

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striking voltage between 30 and 175 kv. A potential divider permits the impulse wave form to be recorded on an oscillograph. (Phillips Electronic Instruments, Dept. Sci121, 750 S. Fulton Ave., Mount Vernon, N.Y.)

■ AMPLIFIER AND TELEMETER are designed as an integrated system for acquisition and telemetering of physiological and psychological data under dynamic and static conditions. Amplifier characteristics are: gain, 4500 single ended and 9000 with differential output; frequency response, 0.4 cy to 10 kcy/sec at 3 db; common mode rejection, 10,000 to 1 or better at 60 cy/sec; input impedance, 0.5 megohm; output impedance, 30 kohm; equivalent input noise with 20,000-ohm source, 4 μv peak-to-peak over bandwidth 0.4 to 60 cy/sec. Telemeter characteristics are: frequency range, 90 to 130 Mcy/sec; sensitivity for 100-percent deviation, 50 to 75 mv; input impedance, 100 kohm or higher; frequency response, d-c to 10 kcy/sec; range, 100 yd, maximum. The two elements may be used in combination or separately; the size of each is 0.8 in^3. (Litton Systems, Inc., Dept. Sci131, 5500 Canoga Ave., Woodland Hills, Calif.)

■ RADIO FREQUENCY MILLIVOLTMETER permits voltage measurement from 500 kcy to 1 kMcy/sec and gives useful voltage indication at frequencies as high as 3.5 kMcy/sec. Full-scale sensitivities range from 10 mv to 10 v. An output is provided to drive a d-c strip-chart or *x-y* recorder. This output is a current proportional to the meter deflection and is designed to operate into a 100-ohm galvanometer. A calibrating control provides for accommodation of other galvanometer impedances. According to the manufacturer, full-scale accuracy of ±3 percent can be obtained from 1 to 50 Mcy/sec, ±6 percent from 50 to 150 Mcy/sec, and ±1 db from 500 kcy to 1000 Mcy/sec. The meter scale is linear. (Hewlett-Packard Co., Dept. Sci122, 1501 Page Mill Rd., Palo Alto, Calif.)

■ RADIATION ALARM is a pocket-sized device worn by the individual to provide audible warning of radiation environments. The device reacts with a violent crackle to radiation levels as low as 3 mr/hr; at 100 mr/hr a buzz replaces the crackle; at 1 r/hr the warning becomes a shrill whistle. Radiation levels lower than 3 mr/hr are indicated by background clicking; the counter operates at levels up to 3000 r/hr. Energy response range is 30 kev to 2 Mev, and efficiency is said to be 98 percent. Two penlight batteries can operate the device for a week. (Gelman Instrument Co., Dept. Sci91, 106 N. Main St., Chelsea, Mich.)

■ CURVE FOLLOWER interprets function curves drawn as a double line with ordinary pencil on common graph paper. The penciled chart, mounted on the instrument's revolving drum, guides a servo-driven capacitance probe that follows the centerline between the plotted lines. A potentiometer geared to the probe-drive mechanism divides any impressed voltage in the same proportion as the drawn curve divides the graph scale. No chart wear is caused by the noncontacting probe. Drum rotation time is adjustable from 300 to 10 sec on standard models. Operation may be continuous or limited to an arbitrary number of cycles by a predetermined counter. Units are available with one, two, or three channels. (Research, Inc., Dept. Sci128, P.O. Box 6164, Minneapolis 24, Minn.)

■ TRANSMISSIBILITY PLOTTING SYSTEM plots automatically the ratio of r.m.s. values of two varying a-c voltages with respect to a third varying voltage. The system covers a 20-db dynamic range with plots appearing on an *x-y* recorder. Voltages at frequencies from 5 to 5000 cy/sec may be plotted; when sinusoidal voltages are used, frequencies up to 20

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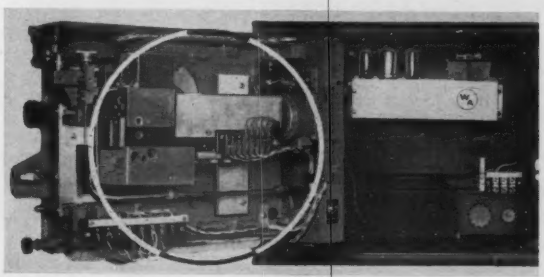

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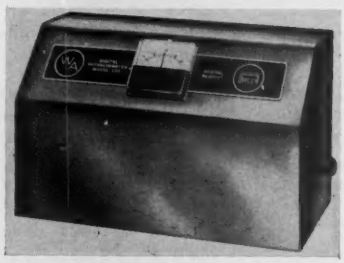

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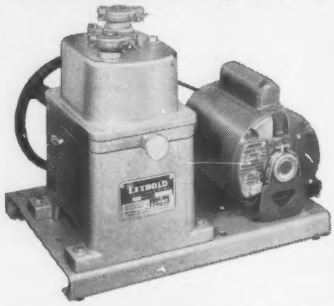

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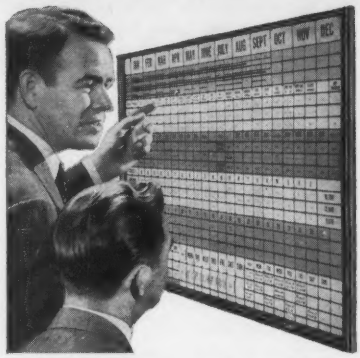

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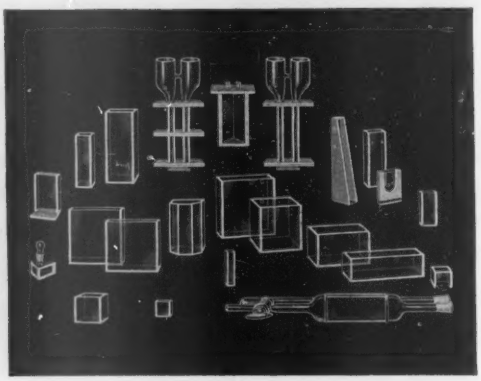

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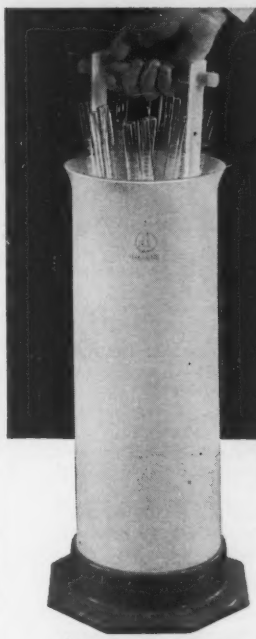

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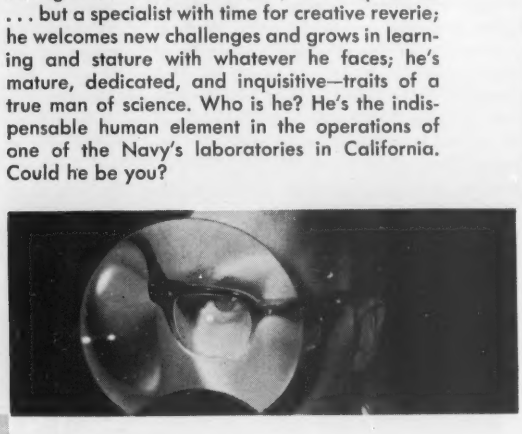

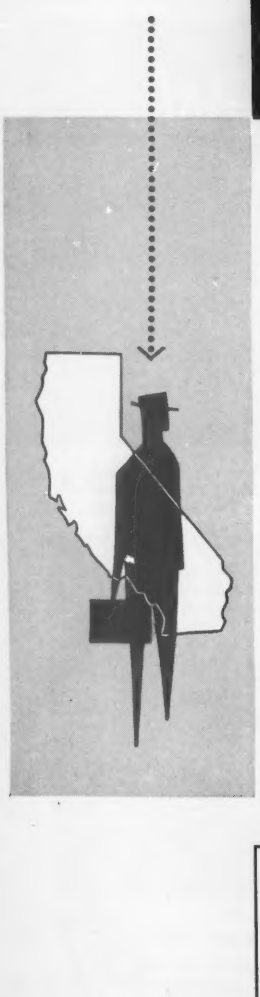

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Letters

(*Continued from page 844*)

that she is no Milton, which might seem irrelevant, except that I believe none but a great poet is qualified to deal, poetically, with the lofty themes she set herself. I will not try to discuss her imagery or style, but I must mention a thoroughly unpoetic trick, of throwing away her climaxes on resounding truisms like "The fecundity of life is to assure survival," or "To birth, death is a force reciprocal." One expects more of poetry than that, and when I say it lends itself to mockery I am not trying to be clever, I am thinking of its effect on the opposition.

As tactics, then, the poem can be described as a distinguished failure, but larger considerations are raised by asking why this failure was necessary—why did the work attempt so much? Its proximate occasion, of course, is the urgent need for preservation of wilderness. That is what the photographs plead for so eloquently, and when I wrote that the mood of the text is relentlessly epic rather than lyric I meant that it protests far too much to serve its obvious purpose. The preservation of wilderness *is* urgent, and it is a moral as well as a political issue, but who are its political opponents? Granted that some of them are short-sighted or mercenary exploiters who might be stirred into "caring enough to act, and to act in time," as Brower puts it, what is a reasonably foresighted developer to make of the call "to learn again to walk with Eden's angels"? More to the point, perhaps, I doubt that a psychiatrist or city planner will be impressed by the lines "How simple our basic needs—/a little food, sun, air, water, shelter, warmth, and sleep!" One can agree that "of all resources, the most crucial is Man's spirit," without necessarily supposing that passage of the Wilderness Bill will alleviate the problems of mental health and juvenile delinquency; in the arena where conflicting political demands can claim equal moral justification the apocalyptic tone of Nancy Newhall's text seems to me poor strategy. It preaches to the converted, and irritates potential allies.

I am sorry that so fine a book, and so nobly meant an effort, should have called forth these curmudgeonly reflections, but their intention is entirely friendly to the cause of conservation, if not to all its methods.

Perhaps I should add that any dispute Brower and I have about "ecologically sound use" of resources must be purely semantic, hinging on the word *ecology*. Otherwise I cannot guess why my definition is "totally inadequate." I include man's spiritual aspirations and his psychic need for solitude and beauty

(as well as for social companionship) within my definition, and I think I also know well the scientific or archival function that undisturbed areas must serve for future ecologists. Presumably Brower reads "multiple use" of wilderness preserves, a slogan of some of his opponents, and he knows, as I do, that by that self-contradictory philosophy Mount McKinley National Park will suffer the fate of Walden Pond. But as a historian of environments, I also know that *absolute* freedom from human disturbance has been unattainable since the Neolithic age began. I am just optimistic enough about human character to suspect that future generations will find their Waldens in places as tame as Thoreau's Walden must have seemed to John Muir. The melancholy fact is that most of them will have to.

EDWARD S. DEEVEY
Yale University,
New Haven, Connecticut

Imprinting

It is regrettable that the term *imprinting* appears headed for the same semantic limbo that *instinct, innate,* and similar, once useful, terms attained some years earlier. This trend toward confusion has certainly not been retarded by the two most recent publications dealing with the subject of imprinting (*1, 2*).

Imprinting has generally been regarded as a somewhat distinctive form of learning (*3*). Its primary characteristics appear to be a restriction of its occurrence to a fixed and relatively brief period in the life of an organism, the absence of overt reenforcement apart from that provided by the subject's response, and a relative stability of the preference that develops for the imprinted surrogate. Hess (*4*) has adumbrated some additional characteristics, though the significance of some of these (for example, differential effects of drugs) is questionable.

Now it should be made clear that all but possibly one of these characters is common to forms of learning that have not, in the past, been considered instances of imprinting. Latent learning characteristically may occur in the absence of overt reenforcement; single-trial conditioning is also not unknown (*5*), nor is the stability of the imprinted response as irreversible as was originally supposed by Lorenz (*6*). The only factor in regard to which one can still assert the uniqueness of imprinting is its temporal fixity: if exposure to a surrogate does not occur within a limited period during the development of the organism—the critical period—the preference for that surrogate does not

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develop. If one argues that the concept of imprinting does not involve the notion of a critical period, limited in time, one can no longer defend the view that there is anything unique about imprinting as a learning process.

I have argued elsewhere (*7*) for the view that intermediate processes link imprinting to conventional types of learning. However, since we do know that some kinds of responses can be established only by exposure to the relevant stimuli during a specific and brief period in the organism's life, and that the response is linked to that stimulus in the absence of overt reenforcement, it does make sense to regard this type of learning as moderately distinct and to call it imprinting.

The papers originally cited, therefore, are deemed misleading on the following grounds.

1) Gray's (*1*) periods of exposure to the model extended for intervals of 24 hours and to ages of up to 5 days after hatching. How he can still assert that he has disproved James's contentions (*8*) when James adhered to our more precise definition of imprinting is difficult to understand. Under normal conditions, one might expect the result obtained by Gray to be attainable at *any* period in the life of the chick. In contrast, James's results can be expected only from chicks of a specific age, the critical period.

2) Moltz's (*2*) efforts to redefine "imprinting" operationally are manifestly pointless. When he ignores his own dicta and continues, in his discussion, to use *imprinting* in a manner differing from his own definition, he compounds confusion.

Finally, I wish to assert that much of the dissatisfaction with the critical-period criterion for the occurrence of imprinting has been assuaged. The difficulty has generally lain in the fact that no two workers could agree on the temporal definition of the critical period. It has recently been suggested (*9*) and demonstrated (*10*) that this has been due to age determinations having been based on the event of hatching (in birds, at least), an event notoriously susceptible to environmental influences. When age determinations are based upon developmental age—that is, time elapsed since the onset of blastulation—no such major discrepancies appear. Thus, it appears entirely reasonable and empirically valid to define imprinting as a rapid form of learning limited in its occurrence to specific developmental stages. That, after all, was what Lorenz (*11*) originally stated.

PETER H. KLOPFER
Zoology Department, Duke University, Durham, North Carolina

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I should like to observe that, besides being a capable investigator, Klopfer has an ability to identify the important points at issue in the theoretical treatment of imprinting. But I cannot agree with some of his criticisms.

First, he seems to object to the fact that I did not refer to a critical period in my most recent article on the subject of imprinting (*1*). The puzzling feature of the objection is that Klopfer did not mention any of the three earlier articles on imprinting of which I was author or co-author, wherein criticality *was* discussed (*2,3*). His apparent inclusion of me among those who do not give proper attention to criticality might therefore be construed as something less than correct. The reason I neglected criticality in the article in question was the absence of appositeness.

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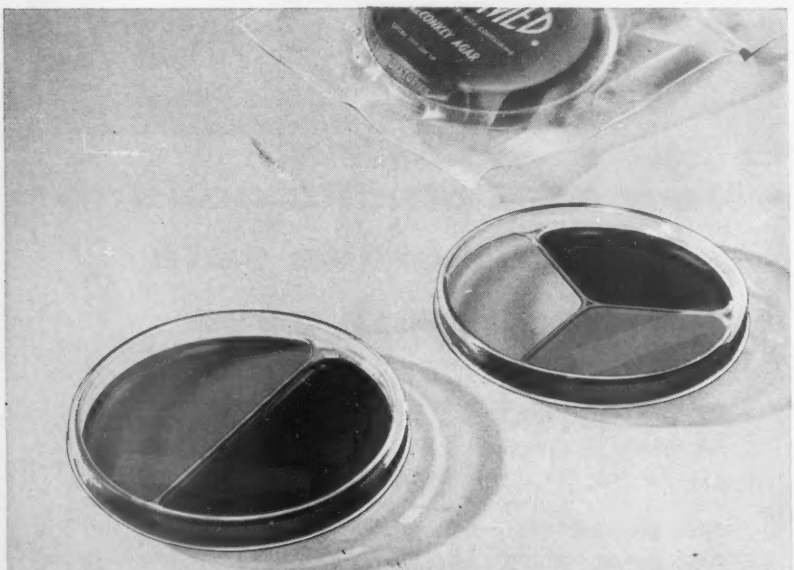

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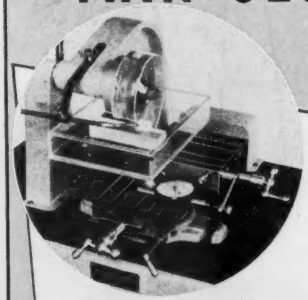

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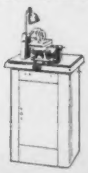

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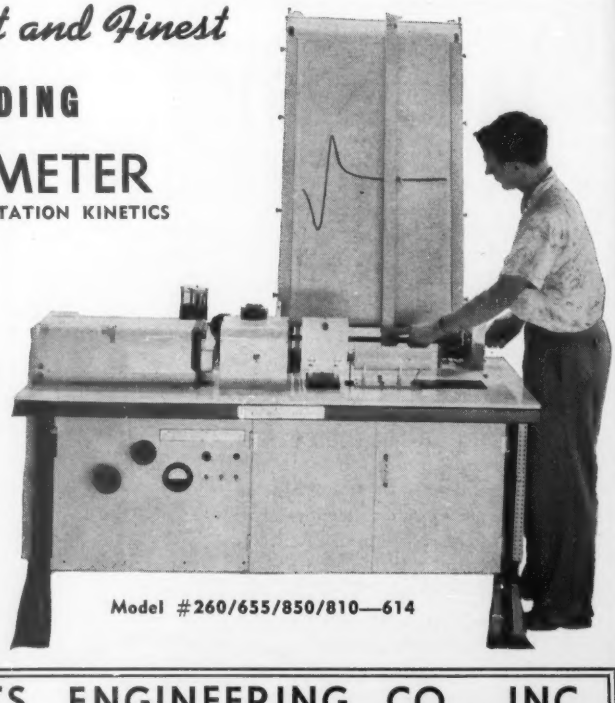

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less objects was aimed at James's (*4*) discussion of retinal flicker in imprinting, a discussion which emanated from what I felt to be a misinterpretation of otherwise perfectly valid data. I have tried, and failed, to understand why Klopfer does not believe my experiment to be a test of James's interpretation. Since my first independent group was started at the age at which James began his dependent groups (experimental and control), Klopfer seems to be saying that I cannot adduce imprinting because I did use independent groups. If my inference is correct, then Klopfer does not consistently hold to his listing of one of the primary characteristics of imprinting as learning in a "fixed and relatively brief period." My period appeared to be as fixed as James's and was even briefer so far as range of age on exposure was concerned; if Klopfer implies that my experiment is invalid because I used a total exposure time per subject of 24 hours whereas James used but a fraction of this, then I haven't the faintest idea what to say except that Klopfer has one opinion and I have another.

Second, the hypothesis which Klopfer advances to account for the differences in critical-period topography from experimenter to experimenter is ingenious and, to a zoologist, undoubtedly plausible. I myself doubt that variation in the onset of blastulation would explain anything more than the subject variability demonstrated when a group is exposed under a certain condition to a certain model, and even here I doubt that the hypothesis is sufficient. This is not the place to present experimental data, but perhaps I may say that I have evidence indicating it is differences in the *models* which produce some, and perhaps most, of the discrepancies in the topography of the critical period.

This finding indicates to me at least, a genetic coding in the animal for reactivity to characteristics of the biologically natural social companion, which we may have been approximating in various degrees with this and that different kind of model. I have evidence that releasers can play a role in the responsiveness of chicks to novel objects, a more significant role than contemporary researchers may be prepared to accept (*5*). I wonder if Klopfer's hypothesis, whatever its validity, does not encourage disdain of the fact that developmental level of behavior can be identified only through behavioral research. We are not yet in that ideal (and perhaps mythical) stage of science where physiological events can predict behavioral events previously unknown. Until that time, and provided Klopfer's hypothesis is not absolutely valid, I suspect that dating the age of a subject from birth (or hatching) will work as handily as dating from blastulation.

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Third, as a zoologist Klopfer ought to realize that much of the reason for dispute about imprinting is that theorists refuse to consider a functional theory of imprinting. It is not that such a theory is nonexistent, because several years ago I published my opinion that the function of the process of imprinting is the establishment of a social bond between the young and its parents, whether in animals or man (*3*). The study of imprinting is the analytical investigation of this process, with emphasis on the behavior of the young. By seizing upon the more romantic elements of imprinting in birds, such as the rapid learning evidenced in some cases and the ability of any researcher to become a Pied Piper of sorts, investigators and theorists alike have ignored the most elementary of all questions in the delineation of a behavioral process: what the process does for the species to help it survive and procreate. Behaviorists may yet regret the day they forgot their Darwin.

Fourth, while Klopfer's allusion to the history of the study of imprinting is by way of being an expository device, I should like to submit a few words about this history, if the rather inadequate knowledge now commonly met with can be called history (*6*). Imprinting was discovered by Spalding, who was also, as nearly as I can determine, the man who first isolated critical periods (*7*). William James gave us our first systematic definition of criticality in behavior and first stated the opinion that the process we now call imprinting is ended by the onset of the fear period (*8*). That aspect of imprinting theory accredited to Lorenz (*9*)—that imprinting involves a rapid learning of the first moving object that the hatchling sees—was previously stated in its essentials by Heinroth (*10*), who apparently mixed long-known research facts with the quite peculiar learning theory of the German philosopher Hermann Samuel Reimarus (*11*). It was Reimarus the Cartesian who originated the conception that lower animals learn what they need to learn in a rapid manner to complement their instincts (compare Lorenz's similar conception in regard to imprinting); from Reimarus's point of view this rapid learning was possible because animals cannot learn very much.

While Lorenz should be given all possible credit for emphasizing the importance of imprinting, he cannot be given credit for a theory the basic outlines of which are not his. Nor should he be given credit for inventing the term *imprinting*, which is a translation of the German term *einzuprägen* used by Heinroth (*10*), which in turn bears strong resemblance to the term *stamping-in* frequently employed by Douglas Spalding (*7*).

PHILIP HOWARD GRAY
Department of Psychology, Montana State College, Bozeman

References and Notes

1. P. H. Gray, *Science* **132**, 1834 (1960).
2. ——— and K. I. Howard, *Perceptual Motor Skills* **7**, 301 (1957); D. M. Baer and P. H. Gray, *ibid.* **10**, 171 (1960).
3. P. H. Gray, *J. Psychol.* **46**, 155 (1958).
4. H. James, *Can. J. Psychol.* **13**, 59 (1959).
5. P. H. Gray, *J. Comp. Physiol. Psychol.*, in press.
6. For example, H. Moltz [*Psychol. Bull.* **57**, 291 (1960)] has reviewed the history of imprinting with practically no consideration of the long list of researchers, from Spalding onward, who investigated the phenomenon of imprinting without express use of that rubric. The names of some of the men who so contributed to our knowledge of this behavioral process would, I feel sure, be not a little surprising to the nonhistorian.
7. D. A. Spalding, *Macmillan's Mag.* **27**, 282 (1873).
8. W. James, *Principles of Psychology* (Holt, New York, 1890).
9. K. Z. Lorenz, *Auk* **54**, 245 (1937).
10. O. Heinroth, *Verhandl. intern. ornithol. Kong. 5. Kong. 1910* (1911), vol. 5, p. 589.
11. The writings of H. S. Reimarus are not generally available, but a critical discussion of his theory may be found in the English translation by his contemporary, C. G. Leroy [*The Intelligence and Perfectibility of Animals from a Philosophic Point of View with a Few Letters on Man* (Chapman and Hall, London, 1870)]. There is also the more sympathetic discussion in G. S. Brett, *A History of Psychology: Medieval and Early Modern Period* (Allen and Unwin, London, 1921).

Klopfer states that my "efforts to redefine 'imprinting' operationally are manifestly pointless." Considering his emphasis on the critical period, I suspect that his dissatisfaction stemmed

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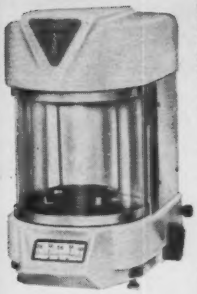

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from the belief that I ignored the "temporal fixity" of imprinting, a characteristic which he regards (and rightly so) as unique. The following definition, contained in the article to which Klopfer refers, makes it evident that I did no such thing: "Thus, imprinting will be defined as the procedure of visually presenting to an animal a large moving object *during the first several hours of its life* under conditions that insure that the object is not associated with such conventional reinforcing agents as food and water" (italics added).

Klopfer also states that I ignored my "own dicta" and that I thereby compounded confusion. I must admit that I am unceratin as to what he intended to convey. To which dicta (or even dictum) is Klopfer referring? What is the nature of the confusion? To what extent have I compounded it?

In conclusion, may I say that it does not appear unreasonable to expect a scientist to be explicit when criticizing the work of another and to offer at least some evidence in substantiation of a sweeping dismissal.

HOWARD MOLTZ
Department of Psychology, Brooklyn College, Brooklyn, New York

Handling Scientific Information

In a recent issue of *Science* [**132**, 1922 (1960)], Helen Brownson, in the article "Research on handling scientific information," makes the following statement: ". . . the essential problem of applying machines to the handling of scientific information on a large scale has yet to be solved. This unsolved problem has to do with means of analyzing the subject content, meaning, and relevance of documents for mechanized handling. Research directed toward this end is making progress but is still in its infancy."

What Helen Brownson calls *the* unsolved problem is really a pseudo-problem which cannot delineate or define a fruitful field for research. In *The Mathematical Theory of Communication*, by Shannon and Weaver, the following two statements appear: (i) "The semantic aspects of communication are irrelevant to the engineering aspects." (ii) "This does not mean that the engineering aspects are necessarily irrelevant to the semantic aspects."

If one properly understands these two statements, one can also understand why mechanized systems and coding can contribute to the semantic aspects of information storage and retrieval systems and why semantic considerations cannot contribute to the solution of problems of mechanization (engineering aspects). Suppose one wished to develop a high-fidelity system

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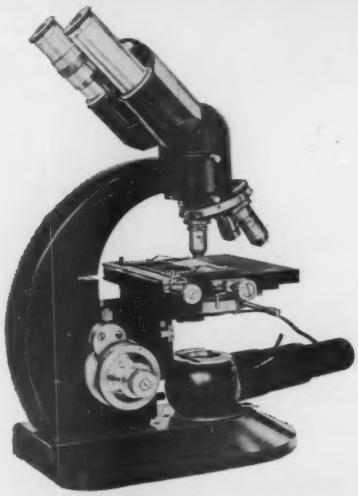

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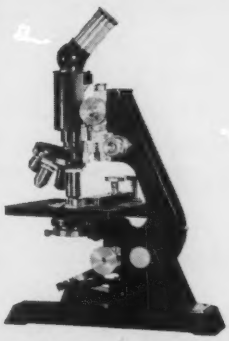

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In brief, the type of research described by Helen Brownson can only be justified by denying Shannon's statement that the semantic aspects of communication are irrelevant to the engineering aspects. This is not usually understood, because this statement is confused with the converse statement—that engineering aspects are relevant to semantic aspects.

MORTIMER TAUBE
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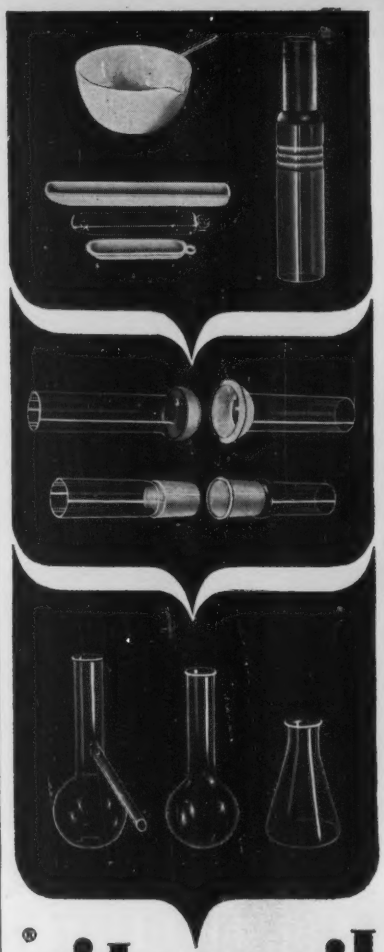
I do not believe the quotations Taube gives from Shannon and Weaver are relevant to the broad problem discussed in my article, which is much more than an engineering problem. In his statement about "the semantic aspects of communication," Shannon was using *communication* in a very special sense—namely, the transmission of messages from one point to another. The paragraph containing that statement begins: "The fundamental problem of communication is that of reproducing at one point either exactly or approximately a message selected at another point" (p. 3). Weaver provides further clarification: "The mathematical theory of the engineering aspects of communication . . . admittedly applies in the first instance only to . . . the technical problem of accuracy of transference of various types of signals from sender to receiver" (p. 97). He emphasizes that the word *information*, in this theory, is used in a special sense that must not be confused with its ordinary usage; in particular, it must not be confused with meaning. At this point, Weaver states, "It is this, undoubtedly, that Shannon means when he says that 'the semantic aspects of communication are irrelevant to the engineering aspects' " (p. 99).

In discussing the interrelationship of the technical, semantic, and effectiveness problems of communication, Weaver points out that the mathematical theory "contributes to the problem of translation from one language to another, al-



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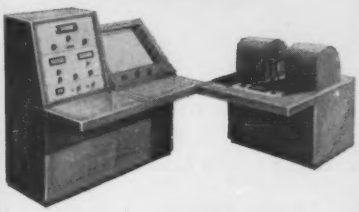

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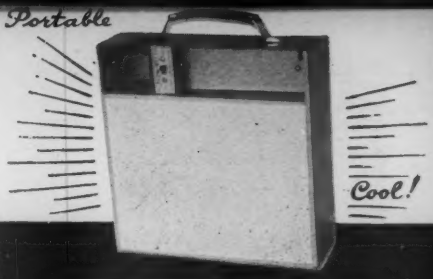

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though the complete story here clearly requires consideration of meaning, as well as of information" (p. 115). Translation is only a part of the broader problem of dealing effectively with the content of scientific literature.

Arguing by analogy is confusing if the analogy is not a close one. With respect to mechanization of the handling of scientific information, the problem is not that of designing something equivalent to a high-fidelity system for the reproduction or transmission of music. We are not merely trying to develop means for undistorted reproduction or transmission of scientific writings. A closer analogy might be the mechanization of some or all procedures involved in handling written music so as to facilitate searches for, say, compositions of a particular period in a particular style, rhythm, and tempo, in which a certain combination of notes is used. The essential problem then would be how best to obtain and to store coded representations of the compositions' characteristics and contents, in machine-searchable form, so that compositions with the desired characteristics could be readily identified—after, of course, first determining what musicologists are likely to want to search for.

I don't know for whom Taube speaks when he says, "We are only interested in storage and retrieval systems because individuals can index material. . . ." It is clear that many persons doing research or administering funds for research in this field believe it worth while to explore the possibility of mechanizing the indexing process or its equivalent. It is important, of course, to work on the engineering problem of efficient manipulation of index data. Such work, however, will contribute little toward the broad problem of mechanizing the retrieval of scientific information if the indexing, whether human or mechanized, is poor. Mechanized information-handling systems will serve us well only if human analysis and indexing of the "input," or whatever mechanized procedures may substitute for them, are sufficiently reliable for scientists to have confidence in the systems.

HELEN L. BROWNSON
National Science Foundation, Washington, D.C.

Doomsday

The article "Doomsday" by von Foerster, Mora, and Amiot [*Science* **132**, 1291 (1960)], although perhaps written and published with an obvious tongue-in-cheek attitude, has received some publicity in the newspapers, and there is danger that it may be taken too

seriously. At least one well-established biological fact has been omitted from the calculations: in human beings it still takes about 270 days from conception to delivery. This fact sets an ultimate limit upon the productivity factor a. If we consider only the reproductive female population (assuming the presence of enough males to maintain the necessary conception rate), it is apparent that the doubling time cannot ever be much less than ¾ year. If von Foerster's equation is valid until this doubling time is reached, the curve at this point has to depart from the power function and revert to an exponential,

$$N = e^{-\alpha t},$$

where α cannot exceed

$$0.69315/0.75 = 0.925 \text{ yr}.$$

From von Foerster's Eq. 12,

$$\alpha = 0.99/\tau,$$

so the power function fails at

$$\tau = 0.99/0.925,$$

or 1.07 years before "dooms-time," when the world population would only be, from von Foerster's Eq. 11,

$$N = 1.79 \times 10^{11}/\tau^{0.99}$$
$$= 1.67 \times 10^{11},$$

a value which corresponds to a population density less than 5 times that of Japan at present. Of course, males and children add something to the problem, but 1.7×10^{11} is far short of infinity, so there is still a ray of hope.

J. S. ROBERTSON
V. P. BOND
E. P. CRONKITE
Medical Research Center,
Brookhaven National Laboratory,
Upton, New York

The essay in doomsmanship of von Foerster, Mora, and Amiot is to be commended. With the exception of their remarks about Malthus, their conclusions are essentially correct. I say this because essentially these same conclusions can be arrived at from Malthusian principles.

People who are without food and water for any extended time die first of dehydration and then of starvation. Hence one can predict with confidence that food and water supplies, F, will limit human populations (deserts, highway U.S. 66, 110°W, 1960). This idea can be expressed symbolically as follows:

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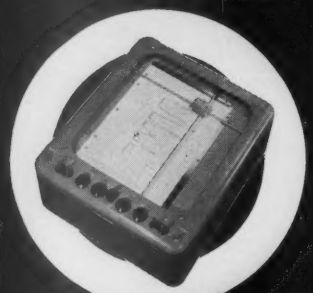

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energy, we can also predict, also confidently, that the limiting rate of food supply is constant. The rate of food accumulation is zero, however, because the limiting population is hungry. It is also certain that in order to reach a limiting population, all surplus food supplies would have to be consumed, or

$$N = N_{max},\ F = 0 \qquad (2)$$

Substituting Eq. 2 in Eq. 1 and integrating, we find, sure enough, that the human population becomes maximal; thank goodness it wasn't infinite after all.

It should be carefully noted that the limiting population may turn out to be larger than that estimated above because people may become smaller.

In conclusion, we should not sell Malthus short. His work in theoretical demography is so nearly contemporary as to make one wonder. There is a solution that has not yet been suggested (except by Swift, for one special case): cannibalism.

WILLIAM E. HUTTON
5133 Waterman Boulevard,
St. Louis, Missouri

In the article "Doomsday," the assumption is made that the fractional rate of growth of population will increase with the population; consequently, as the population becomes larger, the fractional rate of growth becomes larger and before long exceeds the maximum possible rate of increase permitted by the biology of the human species. It seems obvious that such a theory has no relation to reality and is of no value whatever in predicting future populations.

It is possible, however, to use the methods of this article, starting from more plausible assumptions, and to arrive at population growth curves which not only are in agreement with the facts of the past but which do yield helpful suggestions as to how the population will grow in the future. Such a formulation has been made, in accordance with the ideas of Raymond Pearl reflected in Eq. 2 of von Foerster *et al.*

The basic assumption is that the population increases at a rate which is proportional to the product of the population and another term which is equal to the supportable population of the region minus the population itself at that time, all divided by the supportable population at the same time. This is the same as Pearl's basic differential equation except that he calls the so-called supportable population the ultimate population and treats it as a constant. In the new formulation the supportable population is considered to be a function of time—namely, a constant plus another constant times time. The resulting differential equation is easily solved in general form, and curves have been con-

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structed in terms of general parameters which make it possible and convenient to extend the historical data of population of a given city or region into the future. The assumption that the supportable population increases with time is in agreement with the assumption of von Foerster *et al.*—namely, that science and technology do increase the ability of a region to support its population.

Using these theories and the set of curves that have been constructed, we find that the population of the United States agrees remarkably well with the appropriate curve from the family of curves referred to, starting with census data for 1790 and ending with data for 1960. The simpler logistic curve of Pearl fails to give agreement after 1940. One's prediction of future population of the United States depends of course upon the choice of constants, and this in turn depends upon one's estimate of the rate of increase of the ability of our territory to support the future population. Whether this ability increases linearly with time or at a faster rate seems to me to be a matter of conjecture at this time. In any case, such a formulation does offer promise of assistance to those who wish to predict future populations, and the absurd results reported in the article "Doomsday" should not discourage us from making attempts of this sort.

W. E. HOWLAND
Purdue University, Lafayette, Indiana

The article by von Foerster, Mora, and Amiot would be too ridiculous to comment on if it were not such an outstanding example of the inadmissible use of mathematics to prop up a manifestly absurd conclusion. I suppose that the authors are aware of that absurdity, although the tone of the article gives little ground for the supposition, but I wonder why they are not also aware that such articles run the very real danger of increasing the mistrust that many have always shown even of the legitimate uses of mathematics.

The article is so easy to criticize on the basis of the too free use of unsupported hypotheses (particularly Eq. 3) that I shall not do so. Instead, I shall show that *even if the stated hypotheses are accepted the conclusion does not follow.*

It is assumed in the article that the "productivity" α of a population with N members is given by

$$\alpha = \alpha_0 N^{1/k}$$

(Eq. 3), where α_0 and k are constants. The authors then use the "fact" that the rate of change of population is given by

$$dN/dt = \alpha N \doteq \alpha_0 N^{1 + 1/k} \qquad (1)$$

to conclude that N goes to infinity at some finite value (A.D. 2027) of the time.

I wish only to point out that this nonsense does not arise if one only recalls that the size of a population is always an integer. As a result, the expression dN/dt has no real meaning except as an approximation, à fact the authors do not bother to point out. Eliminating this approximation, we see that Eq. 1 should read

$$N(n) - N(n-1) = \alpha_0[N(n-1)]^{1 + 1/k}, \; n = 1,2,...,$$

where n refers to the generation under consideration and the unit of time has been taken as a generation. Recalling that $N \geq 1$ for all n since N is an integer, we see that

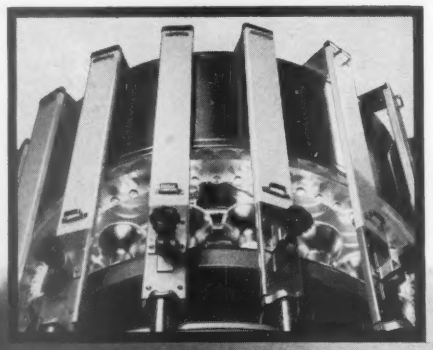
$$N(n) \leq N(n-1) + |\alpha_0| \; [N(n-1)]^{1 + 1/k} \leq (1 + |\alpha_0|) \; [N(n-1)]^{1 + 1/k}$$

Thus,

$$N(n) \leq (1 + |\alpha_0|)^{k[(1 + 1/k)^n - 1]} [N(0)]^{k^n}$$

which is clearly finite for all n.

The argument here should not be misconstrued. The point is not that the world's population growth is not a serious problem but only that progress toward resolution of the problem is in no



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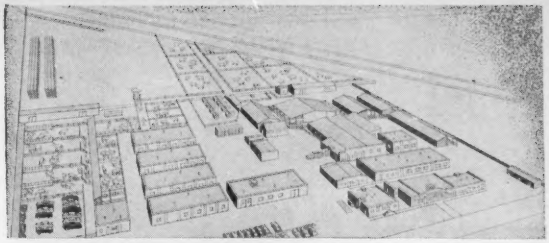

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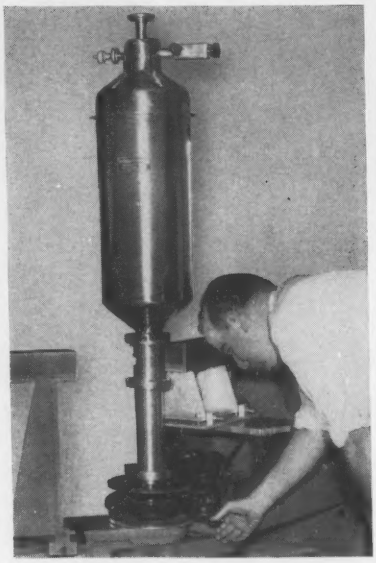

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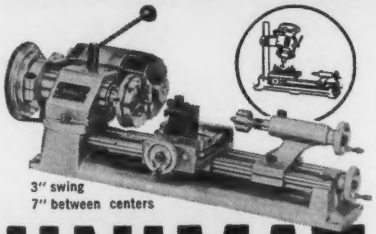

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way served by publication of arguments which, on their face, must be false and which may do some incidental harm. The authors express the hope that their article will "add some fuel to the heated controversy about whether or not the time has come when something has to be done about population growth control." If the article has this effect, it can only be on a controversy among fools.

MARVIN SHINBROT

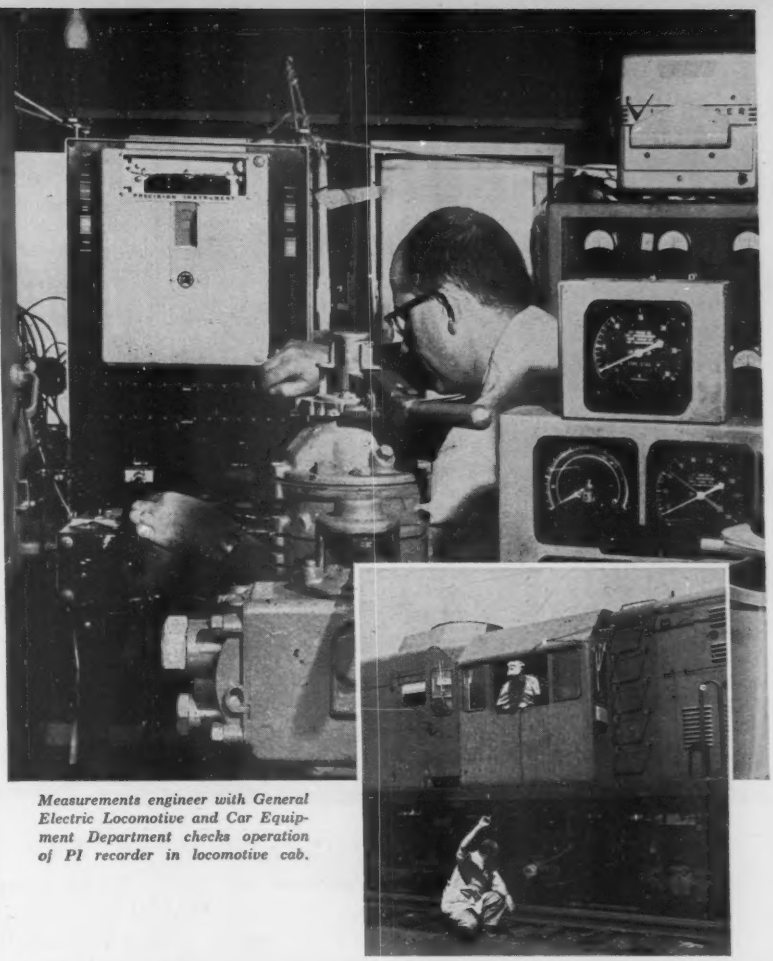
Stanford University, Stanford, California, and *Lockheed Aircraft Corporation, San Jose, California*

We appreciate the opportunity to comment on the remarks which have been made with respect to our article "Doomsday." There are two points which seem to need further clarification. Since we erroneously believed that these points are part of the household furniture of the scientific community, we apologize for having neglected to restate them explicitly. The first refers to the relation between theory and reality, and to the supportability of a hypothesis. We believe that support of a hypothesis is gained through compatibility with experimental observation (*1*) rather than by arguments about what should be the case or what should not be the case. This compatibility establishes the relation between theory and reality and serves as a touchstone for accepting or rejecting a hypothesis. If some of our readers express doubt whether or not our simple hypothesis (Eq. 3) has any connection with reality, we obviously failed to keep them interested in this subject long enough to turn to our Fig. 1, which offers a comparison between theory and observation. Although we know that such a comparison, however favorable, will never prove the "truth" of a hypothesis, we pointed out that it seems that our Eq. 11 may, at least, "serve as an adequate empirical formula for presenting most of our recorded data on human population growth" (*2*).

The second point refers to the interpretation of singularities of the form

$$\lim_{x \to x_0} y = \infty$$

appearing in the description of the behavior of some finite physical systems. Expressions of this form can be found galore. For instance, let x and y represent, respectively, velocity and pressure at Mach 1 (*3*, pp. 3–118); or voltage and current at breakdown voltage in gaseous conduction (*3*, pp. 4–171); or wavelength and index of refraction in optical absorption bands (*3*, pp. 6–63); or temperature and magnetic susceptibility at Curie point in the theory of ferromagnetism (*3*, pp. 4–118); and so on. Physical theory behind these expressions is termed neither absurd nor ridiculous, nor is it customary to deny



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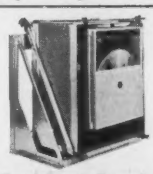

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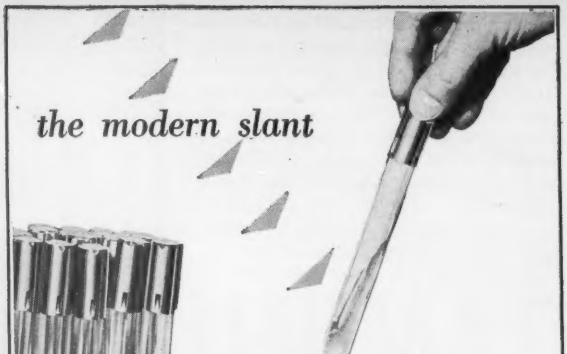

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BIOLOGICAL AND CHEMICAL CONTROL OF PLANT AND ANIMAL PEST

Editor L. P. Reitz **April 1960**

274 + xii pp., 11 illus., 11 tables, 6 × 9, index references, cloth

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that such theories have predictive value because of these singularities. On the contrary, since the generally accepted interpretation of expressions such as these, in which a parameter increases rapidly beyond all bounds, is that the system as a whole becomes highly instable in the vicinity of the critical value x_0 of the corresponding parameter, these singularities serve as welcome warning signals that some breakdown of the system's structure is to be expected.

With respect to the first letter, by Robertson, Bond, and Cronkite, we are very happy to note that this medical research team went along so well with our proposed thesis of "adequate technology," because they obviously must have in mind some tricks for reducing the age of puberty in the human female—the greatest bottleneck in speeding up the rate of reproduction. But who are we to argue with doctors about such points of physiology? However, we may argue their mathematics, because (i) they used a wrong equation for calculating dooms-time for a particular doubling time, and (ii) they failed to follow up their own argument by omitting to calculate the population at doomsday according to the proposed exponential. With our expression for doubling time Δt_2 (that is, Eq. 13, and *not* Eq. 12), one finds the corresponding dooms-time to be $\tau_1 = 2.25\ \Delta t_2$, and invoking Eq. 11, one obtains N_1, the population on that date. With the aid of the suggested exponential we have N_D, the "finite" population at doomsday:

$$N_D = N_1 2^{\tau_1/\Delta t_2} = 3.70 \cdot 10^{11}/(\Delta t_2)^{0.90}$$

With the suggested value of $\Delta t_2 = 0.75$ one obtains $N_D = 5 \cdot 10^{11}$. This corresponds to a population density 15 times that of Japan and about 10 percent that of New York City today. We predicted that this population density would occur on 1 January, A.D. 2024, plus or minus 5.5 years. But according to the arguments advanced by Robertson, *et al.*, we will have this squeeze just 1000 days later. If this is considered to be a ray of hope, the ray is very dim indeed.

We share Hutton's admiration for T. R. Malthus, whose omnipresence in the minds of pessimists as well as optimists we believed we had pointed out.

Howland's suggestion for an approach to population problems is formulated in the differential equation

$$\frac{dN}{dt} = \varepsilon_0 N\ (1 - N/N_0)$$

where ε_0 is a constant and N_0 is the "supportable population." Although this hypothesis may be plausible, it has unfortunately no relation to reality when confronted with estimates of the human global population, unless, as Howland points out, *ad hoc* adjustments for N_0 are made as time goes on. Thus, this theory requires development of a theory for N_0 as a function of t or N. No such function, to Howland's and our knowledge, has as yet been suggested which would fit past data over a period longer than, say, ten generations. In this dilemma we would like to propose, in all modesty, to try tentatively the following, perhaps not too implausible, hypothesis —namely, that N_0 (N), the supportable population, is almost always somewhat larger than the instaneous population N. We suggest:

$$N_0 = N/(1 - \frac{\alpha_0}{\varepsilon_0} N^k)$$

with the constants α_0/ε_0 and k to be determined by observation. We hope that this suggestion meets with Howland's approval, because it catches three flies with one stroke. First, it expresses, in some sense, our principle of "adequate technology," to which Howland has no objections; second, it will enable Howland's proposed differential equation, when properly integrated, to represent human population growth over more than a hundred generations with a mean deviation of less than 7 percent; and, third, it eliminates guesswork about a quantity which is, in principle, inaccessible to experimental observation—namely, N_0, the size of the supportable population. This is easily seen by inserting our suggested function into Howland's proposed differential equation, which leads, after integration and adjustment of the constants by the method of least squares, to our Eqs. 11, 12, and 13, which are free of unobservable parameters. We hope that with this little excursion we have supplied Howland with precisely that formulation

Fig. 1

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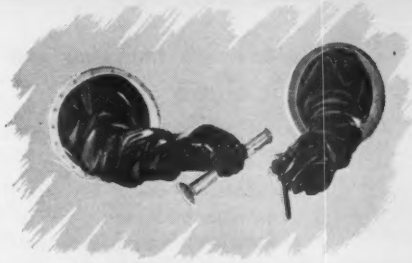

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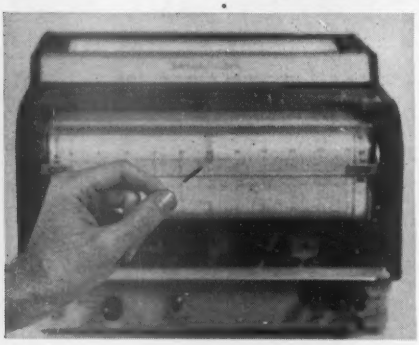

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which, according to him, "does offer promise of assistance to those who wish to predict future populations."

Unfortunately, politeness forbids us to respond to Shinbrot's remarks because this would involve him in a controversy which—in his own words—can only be on "a controversy among fools." Otherwise we would have pointed out our agreement with his feeling that it is unkind to perform a Dedekind cut on a man. On the other hand we could *not* write our differential equation in the form suggested by Shinbrot because we do not know of any integer-triple $N(n), N(n-1)$, and α_0, which would fit for $k \neq 1/i$ $(i=1,2,3....)$, his sugested difference equation. Obviously he must know such triples, and thus his suggested relationship will remain forever "Shinbrot's last theorem."

In the meantime, while we were displaying our wits and know-how in more or less learned discussions about the perennial question of how many angels can dance on a pin point, over ten million real people of flesh and bone, with hopes and desires, with sorrows and pain, have been added to our family of man. Our responsibility demands that we be ready with an answer when these millions ask for their right to live the span of their human condition in dignity.

Let us join forces so that we will not be caught in a dispute seen prophetically by Francesco de Goya y Lucientes: "Of what will they die?"

HEINZ VON FOERSTER
PATRICIA M. MORA
LAWRENCE W. AMIOT
Department of Electrical Engineering, University of Illinois, Urbana

References and Notes

1. B. Russell, *Human Knowledge* (Simon and Schuster, New York, 1948), p. 481.
2. For comparison of our Eq. 11 with estimates of the prehistoric human population, we are grateful to F. Meyer for having drawn our attention to his article "L'Accélération de l'evolution," in *L'Encyclopédie Française* (Larousse, Paris, 1959), vol. 20, p. 24.
3. E. V. Condon and H. Odishaw *Handbook of Physics* (McGraw-Hill, New York, 1958).

History of the Microscope

In Paul Klopsteg's article, "The indispensable tools of science" [*Science* **132**, 1913 (1960)], there are several statements on the historical aspects of microscopes and microscopical discoveries which are inaccurate. In the interest of keeping the historical record correct, I submit the following.

It is considered [see, for example, A. J. Kluyver's notes on Leeuwenhoek's letter to the Royal Society dated 9 October 1676, in *Collected Letters of Antoni van Leeuwenhoek*, Swets and Zeitlinger, Eds. (1939, 1941), vols.

1, 2] that bacteria were undoubtedly observed and described by Leeuwenhoek as early as 24 April 1676, and *not* 1681, as stated. Further, De Waard [see A. Schierbeek, *Measuring the Invisible World* (Abelard-Schuman, 1959)] has discovered that Zacharias Janssen was born in 1588, and his son Hans, in 1611, so that neither could have invented the compound microscope in 1590.

RAYMOND N. DOETSCH
Department of Microbiology,
University of Maryland, College Park

Food Additives

The 27 May 1960 issue of *Science* [**131**, 1581 (1960)] gave editorial approval to the report of the Panel on Food Additives of the President's Science Advisory Committee. The principal recommendation of the panel was to set up an advisory board "to weigh evidence and make recommendations to the Secretary of the Department of Health, Education, and Welfare on the basis of available scientific data on applications for the approval of food additives." In evaluating this recommendation two facts should be considered. First, the panel probably would be under heavy pressure from corporations who would want exemption *now* for additives for which there is *some* evidence of carcinogenic effect in animals. Second, on the basis of present data and techniques, there is no way to make a reliable prediction of the "safe" level of a carcinogenic compound, and—to quote the report—"definitive answers useful in extrapolation to man may not be expected for many years to come."

While the report discusses a number of the major difficulties in the path of scientific decision-making in this area, there is one particular difficulty (which gets bare mention in the report) that we would like to stress here because it is often overlooked. This difficulty arises because (i) the population at risk is of the order of 10^8 persons; (ii) our primary emphasis is on controlling the *number* (rather than the proportion) of cancer cases; and (iii) direct estimates of the risk probabilities would be based on relatively small experiments (10 to 10^2 animals). Since we would be concerned if an agent produced, say, 100 cancer cases, a "safe" level would require risk probabilities of the order of 10^{-6}. Statistical theory indicates that to obtain adequate direct estimates of such small risk probabilities would require a sample of 10^6.

From this standpoint, consider the decision rule: If no cancers develop in 1000 test animals, classify the corresponding level of the agent as "safe."

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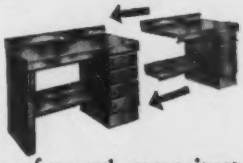

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The probability of obtaining zero cases in a trial sample of size n, when the true incidence is p, is given by the last term of the binomial expansion $[p + (1-p)]^n$, $(1-p)^n$. Thus, if an agent were capable of producing 100,000 cases of cancer in the United States population at risk ($p = .001$), there would be about one chance in three $[(1 - .001)^{1000}]$ that the agent would be classified as "safe." Even if we make the common assumption (which is not always legitimate) that dividing the dose level by 100 would be equivalent to obtaining no cancers in 100,000 test animals, in such a test of an agent which could produce 1000 cases in a population of 100 million ($p = .00001$), there is a one-in-three chance $[(1-.00001)^{100,000}]$ that no experimental tumors would occur.

The present alternative to direct estimation of risk probabilities is extrapolation from dose-response curves. The report states that "dose-response curves for certain potent carcinogens in animals have been worked out from which can be reliably predicted the probability of an individual, in a given size population, developing a tumor from a given dose of carcinogens." This statement requires qualification. While a given technique (such as probit analysis) will often be adequate for ordinary applications (which involve interpolation or very limited extrapolation), the extrapolation required here makes the estimate heavily dependent on the assumption about the underlying distribution (such as the normal distribution). This point is evident when several alternative linearizing transformations (probits, logits, angits, and so on) are used on the same data. While all may provide a fair fit to the observed points and very similar estimates for the LD_{50} (50-percent probability), the *extrapolated* estimates for very small probabilities will not even be of the same order or magnitude. Such predictions are clearly not reliable enough to be used in a decision where human lives are involved.

Until reliable decision-making procedures for the food additive situation are developed—and to develop them is certainly not an easy task—we would question the advisability of vesting an advisory board with power to exempt chemicals that have some experimental carcinogenic effect from the present Food Additive Amendment of the Food, Drug, and Cosmetic Act. An advisory board to review procedure to be considered adequate for testing chemicals for carcinogenic effect in man would, of course, be useful. The creation of such a board probably does not require any amendment to existing legislation.

MORTON L. LEVIN
IRWIN D. J. BROSS
PAUL R. SHEEHE
Roswell Park Memorial Institute, Buffalo, New York

Goals of Secondary School Teachers

As a secondary school teacher (in biology), I feel I must reply to Merritt A. Williamson's letter in *Science* [**132**, 1732 (1960)].

In his statement, "college teaching, as contrasted with secondary school teaching, is concerned with the development within the student of the power to think, reason, appreciate, and discriminate . . . ," he implies that these are not the objectives of the secondary school teacher. He is very wrong. These are the objectives I had when I taught sixth-grade and eighth-grade biology and which I now have in teaching tenth-grade biology. That I am not alone is evidenced in the fact that, through the American Institute of Biological Science's Biological Sciences Curriculum Study program, hundreds of secondary school teachers (among others) contributed to the development of three different approaches to the teaching of biology, all of which embodied these same objectives.

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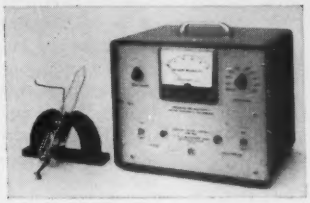

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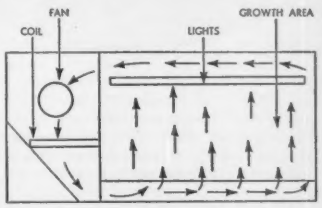

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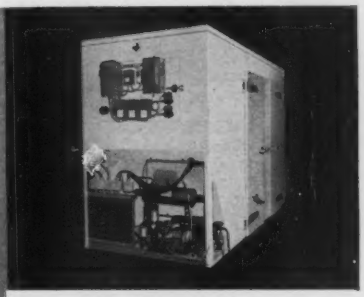

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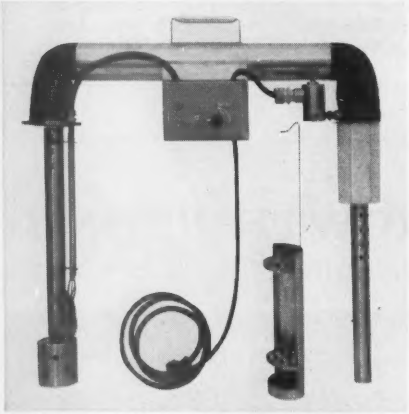

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these three BSCS approaches this year, and I know secondary school teachers in other areas as well as in science who work toward these same goals in their regular teaching programs.

That we do not achieve nearly so much as we would like can be explained by the fact that time is necessary for continuous planning, evaluation, and reorganization of any teaching program as it relates to the individual student and his progress. At the elementary and secondary levels this time is available each day only after a continuous sequence of periods of meeting students in either academic or extracurricular pursuits (periods that often include the noon hour), broken only by the 3-minute interval for changing classes.

Even so, secondary (and elementary) school teachers are concerned and do work toward helping the student develop his ability to think, reason, appreciate, and discriminate. We need, somehow, to provide time for regular professional interchange of ideas in the school day, both within a school system and between school systems, so that all teachers will be stimulated to work more directly to accomplish these aims in spite of many seemingly insurmountable difficulties.

MARON E. STEWART
Ionia High School, Ionia, Michigan

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W. H. Oldendorf [*Science* **133**, 198 (1961)] should be advised that one very good reason for not contaminating books with advertising as he suggests is the very costly increase in postage that results.

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Radiation Exposure

The article by Newell and Naugle on radiation in space [*Science* **132**, 1465 (1960)] is an interesting and timely treatment of the subject. However, it contains several references to ionizing radiation exposure standards for human beings which I feel may be misleading.

A figure of 0.3 r per quarter is referred to as an exposure standard for radiation workers. To my knowledge, this has not been proposed by any group. It probably represents a simple decimal-point slip from the 3.0 rem (close enough to the roentgen for this discussion) per quarter recommended by the National Committee on Radiation Protection and Measurements (NCRP), the International Commission on Radiological Protection (ICRP), and

the Federal Radiation Council (FRC). This error then resulted in the value 1.2 r per year given in Table 4. Neither 0.3 r per quarter nor 1.2 r per year is consistent with the value 5.0 r per year given in the same table.

The long-term occupational dose in the numerical recommendations of the three groups mentioned above is $5(N-18)$ rem, where the individual's age is N and greater than 18. Thus, a person over the age of 18 would be permitted 12 rem every year (3 rem times 4 quarters) until he reached the dose derived by the formula.

The reference to a 25-roentgen "maximum permissible emergency dose" leaves the impression that some serious biological effect will ensue from a higher dose. The article seems to have taken a portion of the NCRP's *Handbook 59* (as revised) out of context. The complete thought is, "An accidental or emergency dose of 25 rems to the whole body or a major portion thereof, occurring only once in the lifetime of the person, need not be included in the determination of the radiation exposure status of that person. . . ."

The NCRP and ICRP are unofficial groups. More recent in origin, and more directly related to NASA, is the FRC, whose recommendations have been approved by the President for the guidance of federal agencies. One recommendation of the FRC would permit a dose exceeding that set forth in the radiation exposure guides after careful consideration of the reason for the larger dose. Surely, a man in space would qualify for consideration.

THOMAS S. ELY
Office of Health and Safety,
U.S. Atomic Energy Commission,
Washington, D.C.

As noted by Ely, our article contains an error in Table 4. The maximum permissible dosage for radiation workers should be 3 r per quarter and 12 r per year, provided the individual's total long-term occupational dose does not exceed $5(N-18)$ r, where N is his age in years. The statement, "In 10 hours a man would receive his allowable yearly dose even with this amount of shielding," should then read, "In 6 hours a person would receive his allowable quarterly dose even with this amount of shielding."

The statement, "After taking such a dose [25 r] the man would not be permitted to take any more radiation in his lifetime," should, as noted by Ely, be deleted.

It was our intent in the article to give the relative orders of magnitude of the radiation levels in space and permissible dosages to indicate the magnitude of the problem presented by this radiation environment. It was not our intent to give the impression that we

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were specifying the permissible dosages for manned space flight.

There are three errors in the second section of Table 2. Item *b* should read: "Electrons, $E>200$ kev: omnidirectional intensity: $\leqq 1 \times 10^{3}$ cm^{-2} sec^{-1}." Item *c* should read: "Protons, $E>60$ Mev: omnidirectional intensity: $\leqq 10^{2}$ cm^{-2} sec^{-1}" (*1*).

HOMER E. NEWELL
JOHN E. NAUGLE
National Aeronautics and Space Administration, Washington, D.C.

Note

1. The symbol $\leqq$ here means "less than or approximately."

Sustained Swimming in Dolphins

Johannessen and Harder, authors of the report "Sustained swimming speeds of dolphins" [*Science* **132**, 550 (1960)], imply that the "length of time at observed speed" (in their Table 1) necessarily represents in each case a time during which the animals swam continuously and unaided at the indicated speeds. It is this implication on which I wish to comment.

Establishing the sustained work capacity of dolphins by the observational methods used by these authors requires identification of the individual animals during the indicated timing periods. The valdity of using groups of dolphins for this purpose is questionable. How can the authors be sure that a group, seen from a quarter of a mile to several miles away, is necessarily made up of the same individuals, or is even the same group, as one seen a few minutes earlier or later?

Part of the problem of proving the marine animal's capacity for sustained swimming at high speed seems to be that of showing that a portion or all of the required energy is not derived from waves. Observations have shown that in some cases no apparent swimming effort is required for dolphins in a bow wave to move through the water at 10 knots (*1*). They have also been seen riding natural waves near shore (*2*).

The numerous observations of "wave-riding" dolphins have been variously explained as resulting from gravity (*3*), buoyancy (*4*), and pressure (*5*)—forces associated with the waves. The question of the origin of the force or forces actually producing the "wave riding" seems at present unresolved.

The work referred to above suggests strongly that observational programs designed to demonstrate the work capacity of marine animals swimming near the surface should give particular attention to waves. The sizes and directions of motion of local wind waves and of swell may be important, especially as they are related to the directions and speeds of motion of the observing ship and of the animals observed.

If dolphins and other marine animals can indeed utilize the energy of waves on the open sea, as well as bow and coastal waves, then the virtual absence of wave data in the observations reported by Johannessen and Harder makes it seem doubtful that these observations can be regarded as clear evidence of the sustained-work capacity of the animals concerned.

A. H. WOODCOCK
Woods Hole Oceanographic Institution, Woods Hole, Massachusetts

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3. A. H. Woodcock and A. F. McBride, *J. Exptl. Biol.* **28**, 215 (1951).
4. W. D. Hayes, *Nature* **172**, 1060 (1953).
5. P. F. Scholander, *Science* **129**, 1085 (1959); A. A. Fejer and R. H. Backus, *Nature* **188** (1960).

In answer to Woodcock's comments we suggest that the questions raised are not applicable to our report to the extent that Woodcock infers. He wonders at our using groups of dolphins instead of individuals. Anyone experienced in shepherding even a well-disciplined group of children will testify that group velocity is equal to and usually less than the velocity of the individual. This

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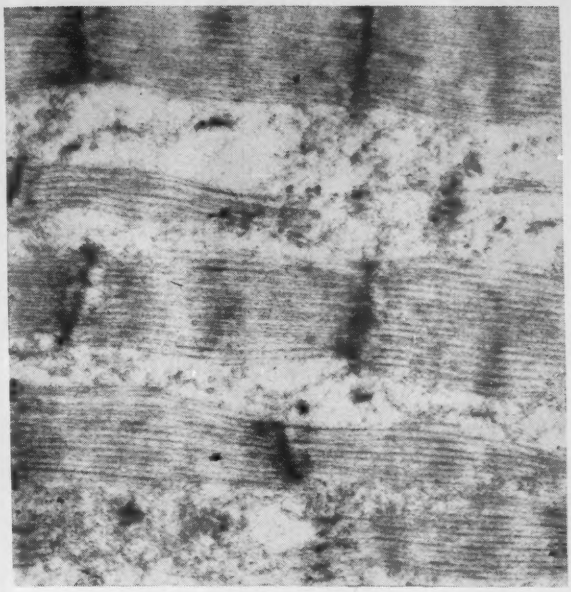

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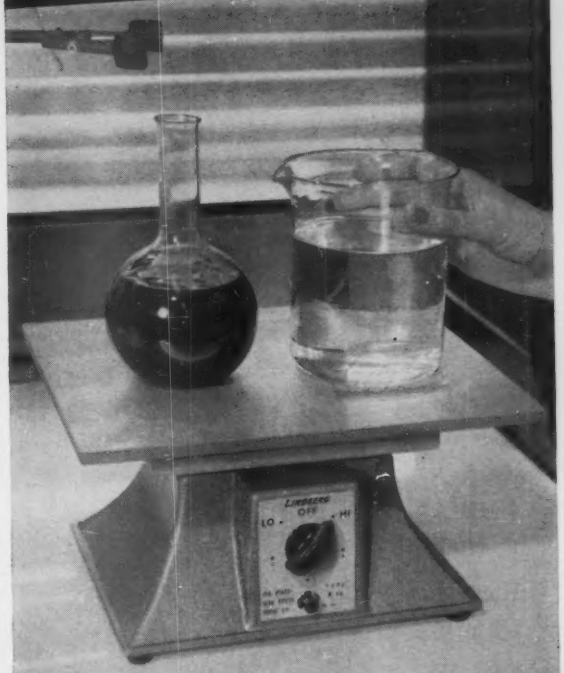

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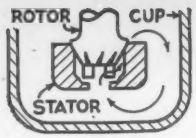

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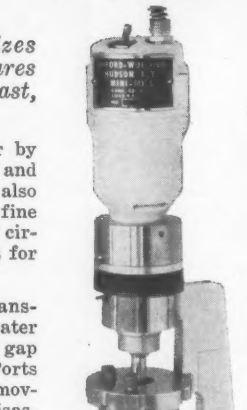

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indicates that the group velocity is a conservative indicator of the maximum capability of sustained swimming speed in dolphins. The infrequency with which large groups were observed is a reasonable guarantee that the observer, Andrews, did not see two disparate groups, at the beginning and end of each observation. A group of 200 individuals swimming at 14 to 18 knots is well delineated by a zone of splashing water.

Although it is well known that dolphins can and do ride bow waves of ships, there is not even unofficial report, to our knowledge, that they can ride the random waves of the open sea. Of course one should not confuse the "lift" a pelagic mammal might obtain from an ocean current with the riding of random waves. The sightings reported by us were made during times of general calm, and during the sighting of the fourth group (200 to 300 individuals) the observer reported an exceptionally smooth, or "glassy," sea.

Finally, it should be pointed out that we are not reporting on the "sustained-work capacity" of dolphins but on their sustained swimming speeds.

CARL L. JOHANNESSEN
Department of Geography, University of Oregon, Eugene
JAMES A. HARDER
Department of Civil Engineering, University of California, Berkeley

Woodcock gives a number of references in support of his conclusion that the origin of forces producing the "wave riding" is unresolved. This conclusion requires examination. He and McBride (*1*) asserted that only the underwater weight of the dolphin could be effective in providing a wave-induced propulsive force, and they reported on their killing a *Tursiops* to find this weight. Subsequently W. D. Hayes (*2*) showed that, from hydrodynamic theory, the forces on the dolphin would be equal to the component of its *total* weight acting parallel to the water surface. More exactly, the component acting is parallel to the surface of constant pressure passing through the animal (the free surface is one surface of obvious constant pressure), for there is no corresponding pressure gradient in this direction.

Woodcock is not alone in believing the hydraulic explanation of questionable validity. Scholander (*3*) proposed yet another mechanism for wave riding and reported on an experiment designed to test the "Hayes effect." He concluded that there was none. When challenged by Hayes to produce data (*4*), he published a figure showing his measurements of the drag on a small fishlike object towed in various parts of a ship's bow wave (*5*). The expected value of the "Hayes effect" force, based on a

reasonable 10- to 15-degree inclination of the equal-pressure surface, was from 110 to 170 grams in this case, in which the object weighed only 650 grams. Although his results were admittedly crude (the scatter in many of the experimental runs exceeded the magnitude of the expected "Hayes effect" itself) and were certainly not conclusive, Scholander in effect invited Hayes to abandon hydrodynamics and to compete with him in the experimental verification of the balance-of-force principle of mechanics. Hayes's explanation is based on such fundamental principles that to deny it is to deny Newtonian mechanics; thus, I cannot agree that the question is "unresolved."

It is curious that whereas Woodcock's original analysis of the forces required for wave riding was in error, his conclusion, that dolphins experience less friction than an equivalent solid body, seems to be true. Quite apart from the evidence of low friction inferred from the unusually high swimming speed of dolphins, mechanical models of dolphin skin made of rubber have been shown to exhibit only about 40 percent of the surface drag coefficient of otherwise equivalent rigid skins (*6*). The present question seems to be, not whether dolphins have an anomalously low friction drag, but rather how low this drag is. Our report of the sustained swimming speeds of dolphins was intended to provide some of the data needed to answer that question.

J. A. HARDER
University of California, Berkeley

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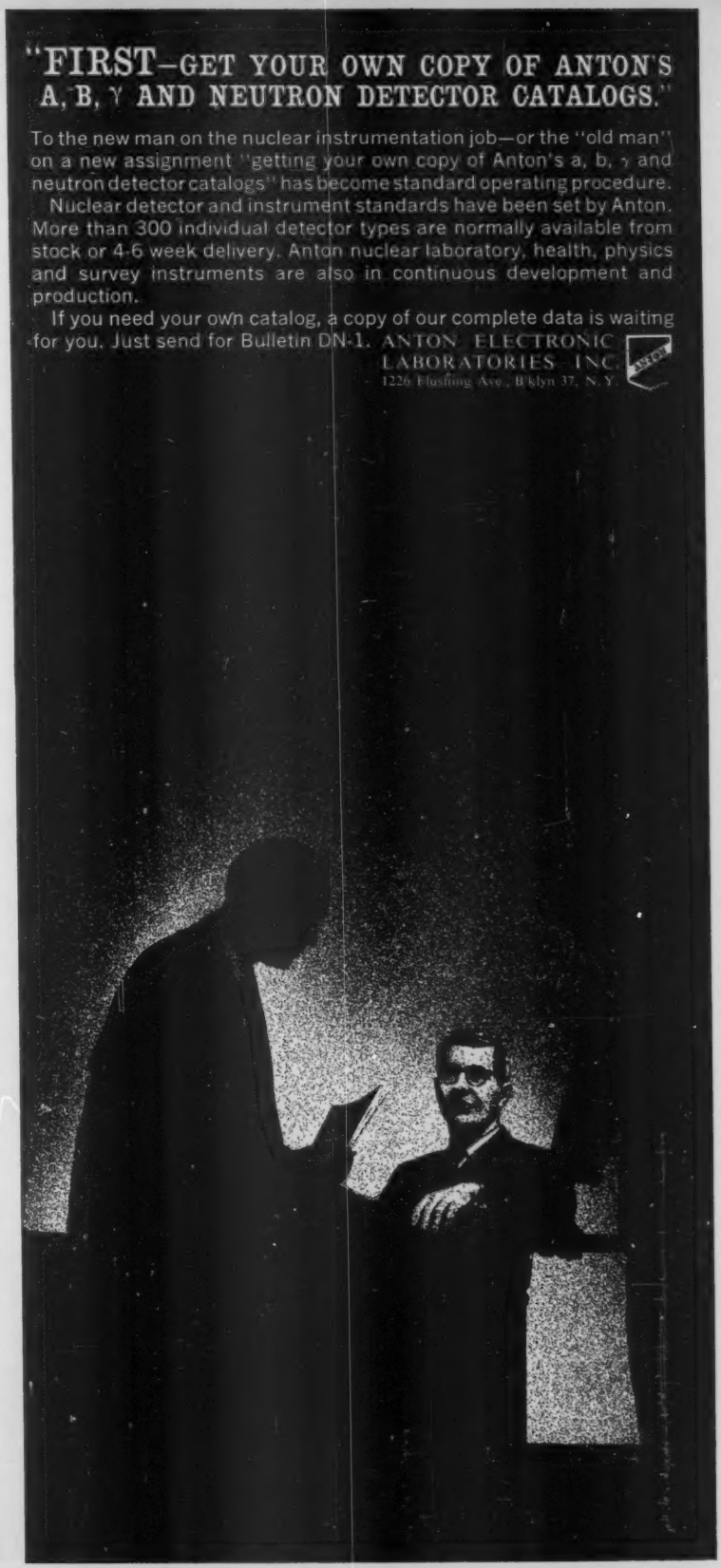
1. A. H. Woodcock and A. F. McBride, *J. Expll. Biol.* **28**, 215 (1951).
2. W. D. Hayes, *Nature* **172**, 1060 (1953).
3. P. F. Scholander, *Science* **129**, 1085 (1959).
4. W. D. Hayes, *ibid.* **130**, 1657 (1959).
5. P. F. Scholander, *ibid.* **130**, 1658 (1959).
6. M. O. Kramer, *J. Am. Soc. Naval Engrs.* **72**, 25 (1960).

Comets

In Thornton Page's report on the Fourth Berkeley Symposium on Mathematical Statistics and Probability [*Science* **132**, 1870 (1960)] there are several points on which I would like to comment.

First, in the legend of Fig. 4, Page states that the semimajor axis of an orbit is one-half the maximum distance from the sun. Instead, the semimajor axis is one-half the *sum* of the maximum and the minimum distances from the sun. Only in the case of the "sungrazing" comets can the closest approach distance be ignored, because it is small in comparison with the maximum distance. In fact, in most parabolic or near-parabolic orbits it is quite impossible to give a good value for the semimajor axis.



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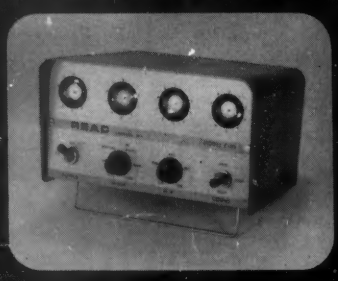

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Secondly, Page says (p. 1875), "Because the space about the sun is so nearly empty, comets move with frictionless ease. . . ." If this were true, one would have to neglect the effect of solar radiation pressure on the comet—an effect which is probably responsible for the anomalous acceleration of some periodic comets after all classical perturbations have been taken into account. This effect seems to be difficult to evaluate for the large mass of data required for a statistical study such as Page described in the report.

Although I have not yet had an opportunity to examine the papers discussed in the article, I feel that the conclusions drawn concerning Lyttleton's theory are unwarranted at this time for the following reason. Most of the comets that can be observed at present or that have been observed in the past have been those that come in relatively close to the sun and to the major planets, and they are probably not a fair sample of the comet population. The question cannot be decided with certainty until observations are made of comets that remain invisible from the earth. Those that do not become visible are objects that miss the sun by a great distance. If one finds that these are much more numerous than the ones that come close to the sun, one might have to adopt the Lyttleton theory. If they are less numerous, the Oort theory would fit the data better. Of course, it is possible that *both* views are correct in a restricted sense. Further research is required on the orbital mechanics and physical nature of comets before any definite conclusions can be reached.

DAVID D. MEISEL
Association of Lunar and Planetary Observers, Fairmont, West Virginia

Meisel is of course correct in his comments concerning the definition of semimajor axis (a) and the effect of radiation pressure on the orbits of comets, but I believe that both are of little consequence. Values of a are so large, and the eccentricity is so near unity for "new" comets, that the difference between $2a$ and aphelion distance is less than one part in several thousand. Moreover, in these orbits of high eccentricity, "new" comets spend most of their time so far from the sun that the effect of solar radiation pressure is limited to two small (and nearly opposite) impulses near each perihelion passage. These amount to a very small bias in the random planetary perturbations considered by Kendall and Hammersley.

In connection with these comments it should be emphasized that Kendall based his study on a carefully selected set of 23 "*new*" comets, excluding of course the periodic comets which, on Oort's theory, have suffered large or many perturbations from their original orbits. Kendall's analysis takes account of the residual observational selection (due to fewer approaches of comets of longer period) but does not concern itself with comets of large perihelion distance simply because neither Oort nor Lyttleton predict large angular momentum of newly formed cometary material about the sun.

The statistical studies I reported cannot be said to disprove Lyttleton's theory or to prove Oort's, as yet, but the limited observational data certainly indicate that "new" comets fall toward the sun from considerably greater distances than Lyttleton's theory would predict. Moreover, Lyttleton's own analysis of the directions of major axes of comet orbits failed to show the expected preference for directions associated with the solar motion relative to nearby stars and interstellar clouds.

The greatest weakness of these statistical studies, as Meisel possibly implies, is the selection of "new" comets for comparison with either theory. Such selection is essential, since random

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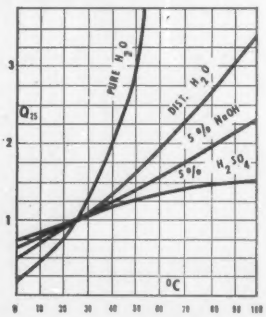

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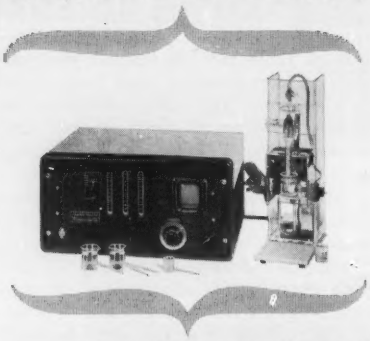

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planetary perturbations soon smear out any record of the original direction or distance of fall toward the sun. Even if we could observe all comets within 10 or 20 astronomical units of the sun, the key to their origin would lie in recognizing the new ones that preserve some record of the initial conditions.

THORNTON PAGE
Department of Astronomy, Wesleyan University, Middletown, Connecticut

Exposures in Lunar Photography

If the errors in *Outer Space Photography for the Amateur*, reviewed by Charles H. Smiley in a recent issue of *Science* [**133**, 271 (1961)], are typified by the example given in the review, they must be "few and . . . unimportant" indeed.

It is well known among astronomers that the full moon is about nine times as bright as the first and last quarters. But since the quarter moon is only half illuminated, the surface brightness of the full moon is only about four and a half times that of the quarters. Thus, the book's suggestion that the exposure for the quarter moon be four times that for the full moon is substantially correct, and the reviewer's "correction," giving the factor of nine, is wrong.

However, to paraphrase the reviewer, if a professional overexposes his first moon photograph, he can make corrections on his second try.

ANDREW T. YOUNG
Harvard College Observatory, Cambridge, Massachusetts

I shall leave my statement as it is, with the factor nine. Young's arithmetic is satisfactory, as far as it goes, but some judgment is needed in addition. The full moon, flat-lighted, is low in contrast; most astronomers expose and develop to increase the contrast. If one is to develop to a high gamma and yet have a reasonable maximum density, one will choose an exposure on the low side, down one or two stops from that indicated by Young's arithmetical solution.

For the moon at either quarter, the situation is different. Then the interesting lunar area is that near the terminator, where the natural contrast is high. One may reasonably choose to expose for the partly illuminated areas and develop for less than full contrast. One might also take into account the fact that the surface brightness of the moon at first quarter is about 20 percent greater than at third quarter.

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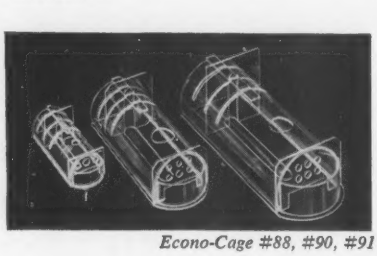

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to worry about overexposing or underexposing by a factor of two. With color, it is different; the arithmetician's solution may not be satisfactory for either the sun-lit or the earth-lit portion of a thin crescent moon.

CHARLES H. SMILEY
Ladd Observatory, Brown University, Providence, Rhode Island

Inquiry into Racial Differences

I agree with the ideas expressed in the letter by Leon S. Mickler on "Racial differences" [*Science* **133**, 202 (1961)].

The proposition that all races are genetically equal in mental abilities has become a part of conventional wisdom, but, in my opinion, none of the supporting evidence meets requirements for proof. It is also unproven that racial differences in mental abilities and achievement have a genetic basis, but it seems to me that the weight of evidence is strongly in favor of this conclusion. The lack of culture-free tests of abilities, problems of sampling and control, and the fact that racial groups are not pure are all barriers to proof. There are methods of studying the problem that have not been tested, and the question could be answered with reasonable certainty, although the procedures would be tedious and costly. We should support inquiry and debate of this question for two reasons. First, science should continue as the free pursuit of knowledge; we should make no rules which stop people from thinking. Second, additional information on racial differences may be required in order for society to work intelligently toward removing the causes of racial problems.

I agree with Mickler that new information on the genetic basis of mental abilities should not threaten the legal or moral rights of any race. It is possible, however, to hold to the principle that each individual be appraised on his aptitudes and behavioral standards without regard to race and, at the same time, to face the possibility that the random mixing of races in schools and housing as a means of achieving desegregation is neither scientifically sound nor morally right. It may well be, if civilization survives and racial bias disappears and each individual is free to move ahead according to his aptitudes and drives, that, although individuals of every race will achieve excellence in every field, there will continue to be important racial differences in interests, aptitudes, and kind of achievement.

DWIGHT J. INGLE
Department of Physiology, University of Chicago, Chicago, Illinois

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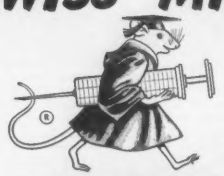

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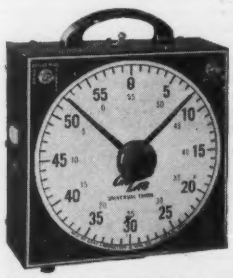

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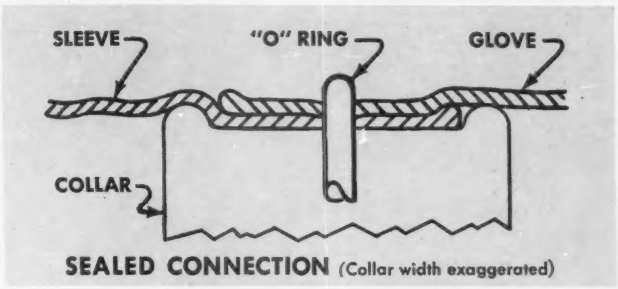

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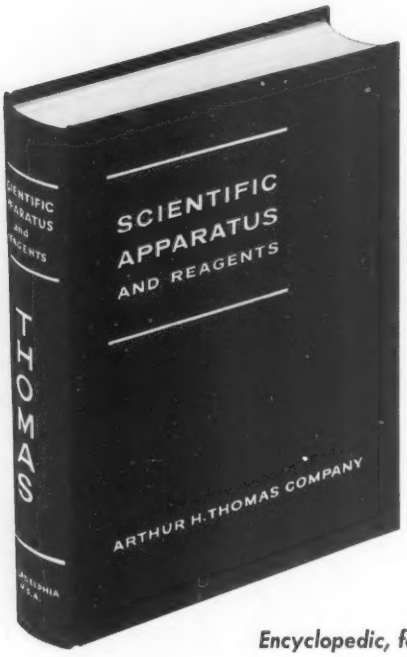

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